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THE UNCERTAINTY PRINCIPLE[1]

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WE have seen that direct experimental evidence pointed to electrons being waves, in the sense that when we send a stream of them through two holes, we can only explain the result by supposing that, like a wave, each electron goes through both holes. We saw, moreover, that if a patch of wave-disturbance in a medium never encounters small obstacles it keeps together as it travels, and behaves in this way like an individual, which is what we think the characteristic of a particle. So we might at first sight be tempted to think that we had got a quite satisfactory and complete view of the character of an electron merely as being a wave of very short wave-length. But a little consideration shows that this will not do.

In the first place we have seen that though a patch of disturbance travels along as an individual with the definite group-velocity, there is always a region round its edges where the disturbance is slowly spreading. There is no way in which a wave can escape this gradual diffusion, and it means that ultimately it will become spread all over space. The rate of diffusion is smaller the larger the volume over which the waves are spread, so that it would be very slow for matter in bulk, and such waves would keep together a considerable time, but still they would not do so forever. Even if we regarded the world as originally created in well-defined "wave-packets," they would certainly by now have spread indefinitely. We may say that the existence of fossils which have preserved their form unchanged for several hundred million years disproves the adequacy of the wave theory.

But the matter is worse than this, since we can do other experiments which seem immediately to disprove the validity of the wave-theory. There exist substances which have the property of *scintillation*

[1] The fourth of the series of lectures on "The New Conceptions of Matter," delivered at the Lowell Institute on March 27, 1931.

when struck by electrons. A scintillating screen is made by lightly powdering a sheet of glass with zinc sulphide crystals; when one of these crystals is struck by an electron it emits a faint spark, which can be seen in the dark with the help of a magnifying lens. When such a screen is exposed to a stream of electrons, scintillations appear irregularly all over it. The natural inference from this experiment is that the stream is like a shower of rain falling on the screen, and each scintillation is produced when a single drop hits the screen. We seem to have a perfect and complete proof that the electrons are little bullets each traveling along a line from source to target.

It looks as though we had arrived at a flat contradiction. This experiment tells us that the electron is a bullet in one part of the stream, while we could not explain Thomson's experiments without supposing that the electron went through two holes at the same time, as only a wave can do. To bring out the contradiction still more strongly we may combine both experiments into one; though this experiment has not actually been done, there is not a shadow of doubt what would be found if it were practically possible. If we sent out a stream of electrons through two small holes close together and then looked for scintillations, we should find these still appearing as isolated sparks, but the sparks would all occur in certain bands, and none at all in between at the places where the diffraction theory predicts darkness. But if we afterwards block one of the two holes, we shall destroy the interference and shall get scintillations everywhere. The crude way of saying what has happened is that the electron stream was a wave when it was going through two holes, but has miraculously turned itself into a particle when it hits the screen. Of course such a description is not to be tolerated, since it would imply foresight on the part of the electron as to what was expected of it. We can imagine, for instance, that we could swindle the electron by pretending we were going to put a shutter with holes, so that it should get ready to be a wave, and then put a scintillating screen instead. Absurdities of this kind show that we have arrived at a very fundamental difficulty.

The elucidation of this contradiction is really the central point of the new quantum theory. The explanation, due to Heisenberg and Bohr, starts by showing that in fact the properties are not contradictory but complementary. Whatever the thing is that we call matter, it can be submitted to various experiments, some of which are devised to show wave properties and some particle properties; but if we devise an experiment which shows the wave properties, that experiment debars us from observing the particle properties at the same time, and *vice versa.* Suppose, for instance, that we wanted to make sure that it really was particles that were going through our holes. We should set a scintillating screen over one of them, and whenever we saw a scintillation we should say that there was a particle coming to that hole. But in doing so we should have prevented the particle going through and so obviously should not get interference on the other side. Next we might try to improve the experiment, by imagining our screen was so thin that the electron could produce a scintillation on it and still get through. Could we not then get interference between this part of the electron wave and the part that went through the other hole unimpeded? We should fail, because, though the electron wave has got through the first hole, the mere act of exciting the scintillation will alter the phase of its wave, and if this phase changes there can be no interference. We have laid a trap for the electron to induce it to tell us which hole it went through, but when the electron answers the question that it went through one hole, it automatically refuses to do the interference which would confess that it went through both.

It is the recognition of this and similar facts that has cleared up the mystery of the quantum theory. A situation arose rather like that in the early days of the discovery of relativity. The great idea which Einstein contributed to scientific philosophy was the principle that if a thing is essentially unobservable then it is not a real thing and our theories must not include it. He showed how the idea of absolute time was of this nature, and the whole beautiful structure of relativity was built up from that basis. But a self-consistent mathematical formulation of the theory is not enough; it is also necessary to convince ourselves by examples that in fact it really is impossible to determine whether two events in different places occur at the same instant. We learn to understand the theory much better by "shamming stupid," trying to lay traps for the theory and seeing how it escapes from them. Much the same state of affairs has arisen in the quantum theory; we have considered one case where we laid a trap for the electron, trying to make it tell us whether it was wave or particle, and we have seen how it avoided the trap. We must convince ourselves that no experiment can be invented which should at the same time require the electron to behave like a particle and like a wave. The guiding principle which establishes this result is called the uncertainty principle, and we shall discuss this and with its help shall see how the conflict between wave and particle is always avoided.

As we have seen, some experiments with electrons exhibit their particle characters and some their wave characters. We can not avoid thinking about both, and it is a very confusing thing to have to do. In

one picture the electron is a little speck of dust, or a bullet, and in the other it is, shall we say, like a stormy sea, and it is not easy to see much resemblance between the two. There have been attempts to regard it as a speck of dust *in* a stormy sea, but it can definitely be said that they are of no use at all. Perhaps the best description can be made by the use of a commercial expression; an electron is a particle "and/or" a wave. We must be ready all the time to think of it as either or both, but we must not mix the ideas. There are two half-worlds, each of which gives a partial view of the whole world; they are related to one another and interdependent, but they are expressed in different languages. We call the two half-worlds the particle aspect and the wave aspect. There is nothing of the same kind anywhere else in scientific thought, but the absolute separation, yet interdependence, can perhaps be compared to a similar separation in metaphysics. There is a close interdependence between the objective thing that we see or hear and our subjective sensation of sight or hearing, and yet the two use wholly different languages. When a string on the piano vibrates 256 times a second, we hear "middle C" without any conception that there is anything happening 256 times; and when an ether vibration with twenty thousand waves in each centimeter strikes our eye, we see yellow, an ultimate sensation giving no hint of a wave motion. In the same way, when we burn a finger in the objective world it is because the atoms of the fire are moving about a little faster than those in ourselves, but actually all we feel is that it is too hot. There is the same kind of interrelation without identity between the wave and particle aspects of matter. It is tempting to carry the analogy a little further, and to decide which way round it is to be taken. I think that it is best to regard the wave aspect as analogous to the objective world, and the particle to the subjective; for example, we have a very direct and intimate perception of what a particle means if we are hit by a bullet, and, on the other hand, we have no intuitive knowledge whatever that light and sound have anything to do with waves. But I do not in the least want to insist on this; the whole thing is only an analogy, and perhaps some will say a fanciful one. I am too bad a metaphysician to judge of this.

We must consider a little more closely the interdependence of the two aspects. In the last lecture we saw that it had been shown that under certain conditions the wave of an electron would have wave-length about the same as that of x-rays, that is about a hundred millionth of a centimeter. Thomson and others have experimented with electrons which, regarded from the particle aspect, have various speeds, and have found that the wave-length is inversely proportional to the speed; but the limitations of experimental technique prevent the investigation of any very wide range of speeds. Theory, however, clearly indicates what the relation will be between speed and wave-length; indeed the experimental work was really only a verification of the theory. The relationship was first given by de Broglie, and involves the *quantum* itself. The quantum is a certain universal constant which is always turning up in atomic theory. That it is a perfectly genuine quantity is shown by the fact that it has with some precision the value 6.545×10^{-27} gr. sq. cm per sec., but this does not really help any one to understand it. Its nature is best described by saying that it is the single universal connecting link between the particle and wave aspects. The rule for finding the wave-length of any particle is to divide the quantum by the momentum of the particle, and this gives the ultimate meaning of the quantum. The rule is true not only for electrons, but also for protons, atoms, molecules, photons and even bodies of ordinary large size.

In order to observe the wave aspect easily, we want to get long waves, and that means small momentum, and small momentum can be got either by having low velocity or else very light particles. For this reason most experiments on the diffraction of particles have made use of electrons, the lightest particles that exist. It is interesting, however, to note that recently the diffraction of whole atoms has also been observed. We will consider a few of the associated values for electrons of speed and wave-length, but in doing so, it must be strongly emphasized that we are describing the two irreconcilable aspects of matter as though they could be mixed together. When I say that such and such a speed implies such and such a wave-length, it is only to be taken formally. It means that if we have a suitable grating, lateral spectra will be found corresponding in position to that wave-length.

In Thomson's experiments the electrons were set in motion by an electric field of about 20,000 volts. This gives them the high speed of 8.5×10^{9} cm per sec., more than a quarter of the speed of light, which would carry them right round the earth in half a second. The associated wave-length is 0.8×10^{-9} cm, about a twentieth of the distance between the atoms in the analyzing crystal. In Davisson's experiments much lower voltages were used. With 200 volts the speed would be a tenth as great and the wave-length ten times as much, nearly as great as the size of the atoms of the crystal. For much lower voltages the experiments would become very difficult both because the electrons produce hardly any observable effect, and also because they will not be diffracted when their wave-length is greater than the interatomic distance. It is very probable that these difficulties will be overcome in time; indeed a beginning has already been made in that it has been found possible to observe

the diffraction of electrons, though still rather fast ones, with an ordinary optical grating instead of a crystal. Though experiments are lacking, theory predicts with confidence the wave-lengths associated with slower electrons. An electron moving at the speed of a rifle bullet has wave-length about a thousandth of a millimeter, a length visible in a microscope. An electron moving at the rate of ordinary human walking would have wave-length of a size just visible to the naked eye, and one moving at the rate of a rather slow tortoise would have wave-length an inch long.

These relations are so far only formal. We do not expect to be able to see the crests of the waves or anything of the kind, but are only maintaining that certain experiments, at present quite impracticable, would reveal diffraction effects which would imply these wave-lengths. But let us take the relationship more literally and see what it implies. An electron-particle moving at a rate of one centimeter a second is an electron-wave of length seven centimeters. Now a wave of seven centimeters does not by any means signify a wave with only two crests seven centimeters apart; it means an infinite train of harmonic waves stretching to infinity in both directions, with all the crests regularly arranged at intervals of seven centimeters. Where, then, is the electron particle? The answer is that it may be absolutely anywhere! This was the key to the elucidation of the whole quantum theory; it was entirely unforeseen and it is the central fact of the new conception of matter. Let us examine the question in more detail. Perhaps we have taken a rather too pedantic view when we say that the mere calculation of a wave-length implies that there was an *infinite* train of harmonic waves, for after all a train of waves with twenty or thirty crests travels for a time in much the same way and could show diffraction. Such a group or *wave-packet,* as it is often called in the present connection, travels along with the group velocity, but spreads a little as it goes. Where is the electron-particle now? The medium carrying the electron wave is undisturbed, except where the packet is, and so we can say that the electron particle is at all events somewhere in the packet, but we do not know whereabouts in it. The packet moves with the group velocity, and the electron must keep in the packet, so it must move at something of the same rate too. But now there enters the important point that a wave-packet always spreads, and so at a later time is longer than at the start, and therefore there is a wider region available for the particle-electron. This can be expressed in another way; we may say that the speed of the particle is not exactly the same as the group velocity of the waves but may be a little more or less. For example, if the particle is at the hind end at the beginning and at the front end of the wave later, when it has spread, then it will have gone faster than the group velocity. On account of the spreading of the wave-packet there is an uncertainty of the speed of the particle. The point of the new outlook is that though we think of a particle as associated with the wave, it is *impossible* to know where in the wave it is, and *impossible* to say exactly how fast it is moving. Our first tendency is to resist this conclusion and to say that we can imagine ways of finding where the electron really is and how fast it is moving. We shall consider this point soon, and show how such an experiment is always defeated, but it will be best to accept it for the moment, simply taking the rule that the particle-electron is somewhere in the wave-packet, and consider what degree of uncertainty of position and motion this implies.

The uncertainty of position of the electron depends on the size of the wave-packet, so that for a long packet, containing a great many crests, the position of the particle is very uncertain. Such a wave group on the other hand does not spread very rapidly, and so we can say that the velocity is rather precisely given. Next consider the opposite case of a very short wave-packet. In such a case the spreading is very rapid, so that the velocity is very uncertain. The general result thus is that the greater precision we demand for either position or velocity, the less the precision that can be assigned for the other. The rule is more definite than this, and can be given a rough numerical value. The product of the uncertainties of position and momentum of any particle can not be brought below a value equal to the quantum. This is true for all particles, electrons, protons, photons, atoms, and so on. It is the uncertainty principle.

The relation between wave-length and momentum is only one way in which the wave and particle aspects are connected. There is another which in many ways is quite as important, and which must be described. We may recall that the character of a harmonic wave depends on both wave-length and wave-velocity, and that from these two a third can be devised, the frequency, which is the number of oscillations per second described by the medium at a fixed point. The frequency is really the most fundamental of the three, for if the medium has variable properties the wave-length and wave-velocity will vary in different places, but the frequency will be the same everywhere. The frequency of an electron belongs of course to its wave-aspect, and the corresponding quantity in the particle aspect is the energy. The energy can be derived by multiplying the frequency by the quantum. There is also an uncertainty principle for the energy, just as there is for the momentum. This asserts that if we want to measure energy accurately we must take a long time in order to do so. If, on the other hand, we want to know the energy

at a certain moment, we must obviously only use a short interval of time round that moment to do the measurement, and the value we obtain will be inaccurate. It is easy to see why this should be so by taking account of the wave aspect. An accurate knowledge of the energy implies an accurate knowledge of the frequency, and this knowledge can only be attained by letting the oscillation run through a great many cycles, that is to say by taking a long time.

We have seen how the uncertainty principle arises quite naturally from the behavior of wave-packets. But we must now assure ourselves that no experiment can be devised which would directly determine both position and speed with a higher accuracy than the principle permits. In the first place a simple calculation shows that bodies of ordinary size, on account of their great weight, have so little uncertainty of velocity that the ordinary disturbances of the world will far exceed it. The effect only becomes perceptible for particles as light as atoms, and the most favorable case of all is the lightest particle, the electron. Let us therefore imagine that we have a skeptical experimenter, who refuses to believe in the wave theory, and sets to work to show that he can fix the position and speed of an electron at the same time with as high accuracy as he pleases. To make his experiment easier he will take the electron to be at rest, but it should be mentioned that this has nothing to do with the uncertainty principle; for that principle the difference between an electron at rest and moving at a centimeter a second is just the same as the difference between one moving at a thousand centimeters a second or a thousand and one. Our experimenter claims to have got an electron precisely fixed and at rest. We will cross-examine him about his work and see what he has found.

Q. How did you know the electron was there?

A. I saw it.

Q. An electron is a pretty small thing and not easy to see. How did you manage?

A. I had a microscope.

Q. Even a microscope can only see things of the size of a wave-length of light. You can't be much of a precisian if you say you knew exactly where it was from that. I thought you said you would guarantee to know *exactly* where it was.

A. Yes, but you see I had taken a course in optics at the University, and so I was not caught out as easily as that. I invented a special X-ray microscope. It has a wave-length of a thousand millionth of an inch. Of course there are the cosmic rays with still shorter wave-length, but nobody seems to know where they come from, so they would not be very handy. Any how I think I have done fairly well.

Q. Well, I haven't yet heard of an X-ray microscope on the market, but I suppose there will be one soon. Perhaps it would be pedantic to want you to do better. What did you see?

A. It was rather tiresome to get it going, but when I had done so an annoying thing happened. I knew the electron was there or thereabouts, because I had put it there; and it was at rest because otherwise it would have gone off while I was getting the microscope ready. Well, I was adjusting the microscope, and the electron was coming into focus beautifully, when it seemed to give a jump and run away. So that experiment was spoilt and I had to start again.

Q. Did you have better luck next time?

A. No. It was most curious; exactly the same thing happened every time. I think there must be something wrong with the microscope stage. I am going to have a shot to improve it. But as the microscope was certainly right in principle for seeing things to a thousand millionth of an inch, and as the electron stayed there all the time I was focussing it seems to me that I must be right. It is only a matter of overcoming the troublesome details that turn up in all experiments.

Q. It is not a matter of troublesome detail and there is nothing wrong with your microscope stage. Your trouble is not with the electron being there and staying there, it is with the seeing of it. You can't see the electron without light to see it by, and the light disturbs the electron and drives it away. It does not matter how many different experiments you design, you will always get caught out in one way or another. There is no escape from the uncertainty principle.

The old particle theory breaks down not because it is inconceivable to imagine a particle at rest at a definite place, but because every method that can be contrived to *observe* that it is there always introduces a disturbing element. The ordinary experiments with gross matter are made with instruments so designed that they do not perceptibly disturb the object measured. It would be a poor way of measuring the length of a stick to hit it with a steam hammer, and if we want to see what a microbe looks like we do not place it in the focus of a powerful burning glass. The measuring instrument is always chosen lighter or weaker than the object measured; but this can not be done when the object is the lightest thing that there is, an electron. In designing the experiment which is going to observe the electron we have to examine all its details so as to be sure that the method of observation is not going itself to introduce some disturbance. We do not of course expect anything as crude as the burning of the microbe, but we must estimate what effect there may be. We shall find that the effect exactly explains poor A's troubles, but in order to do so must make a digression.

It was known as early as the eighteenth century that all forms of wave exert a pressure on any obstacle that is reflecting them. This can easily be seen with a stretched string. Instead of tying the string to a support at the right-hand end, suppose that it

passes through a hole in a frame and is made fast somewhere beyond. The string just fits the hole, and the frame is held firm. When a wave of vibration travels along from the left towards the frame it can not pass the hole but is reflected back, forming "standing waves" with the hole as one of the nodes. Now consider the forces acting on the frame. The string has a bend at the hole (except at the moments when the phase of the wave makes it straight), and the frame has to bear the pressure of this bending. The direction of the force evidently bisects the angle between the string on the two sides of the hole, and so is nearly sideways, but not quite so, for the bisector must always fall to the right a little way behind the plane of the frame. The principal component of the force which is in the plane of the frame is alternately in opposite directions and so averages out; but the longitudinal component is to the right whether the vibrating part of the string is up or down, and so there is a residual force to the right on averaging. If we do not wish the frame to move, it will be necessary to hold it with a small force pushing towards the left, the direction from which the waves are coming. This means that the waves exert a pressure on the frame. More detailed consideration shows this pressure to be proportional not to the amplitude, but to the intensity of the waves.

It is found to be a universal rule that waves of every kind exert a pressure on an obstacle reflecting them. This must therefore be true of light, and the effect was predicted and many of its consequences were worked out long before the phenomenon was observed. The effect is very small indeed for any available source of light—the total force exerted by the sun shining vertically on a square mile of the earth is equal to a weight of about 3 lbs. The first attempt to detect the effect had a rather surprising result. Crookes made a little wind-mill with vanes blackened on one side and polished on the other. The polished sides reflect light, while the black absorb it. In consequence the force on the black side is half as great, for though it is receiving the wave it is not returning it. When exposed to a bright light, the radiometer should therefore go round with the black sides leading. It does go round, but the other way! This was ultimately traced to an effect of the irregular heating of the residual gas in the vessel; though very small it still far outweighs the minute direct effect of the light. It is only in comparatively recent times that this difficulty was overcome, first by Lebedev in Russia, and the actual pressure of light directly observed.

The pressure observed in this way is the gross pressure observed on the whole of a body in bulk. This must be regarded as the result of all the separate pressures on the atoms and electrons. The simplest inference that could be made was that each electron just took its proportional share of the whole. But with the development of the quantum theory it became possible to admit that this might not be so. If, for example, a few of the atoms got a violent kick and the rest none at all, the cohesive forces of the material would enable the few to drag the many with them, and the result in bulk would be just the same as though all the atoms had experienced a feeble force. This was the guiding idea in the very important discovery by A. H. Compton in 1922. From general considerations of the quantum theory as it then was, Compton put forward the idea that when light falls on an electron the process should be regarded as though it were a collision between two particles. Remember that this was before any one dreamed of the wave aspect for matter, and though the particle aspect of light was well known, no one before had ever dared to take it in anything like as literal a form. With the details of the Compton effect we shall be concerned in a later lecture. Here it suffices to describe the outline. When an electron scatters light it is thereby caused to recoil and the speed of the recoil depends only on the wave-length of the light and not at all on its brilliance. For visible light the recoil is feeble, but for x-rays it becomes very easily perceptible, and in fact Compton verified his theory in all its details by using x-rays. The only distinction between the effect of a bright light and of a faint one is that bright light will scatter an electron sooner than the faint, but the speed at which the electron goes will be the same in either case, provided the wave-length of the light is the same.

We may now return to our experiment with the microscope, and we know where the trouble lies. A microscope system consists of two parts, the condenser and the microscope proper. The condenser focusses light on the object, the object scatters it, and the microscope then refocusses into the eye. If we are to see an object, that object must have scattered light, and must itself recoil in consequence. So the mere fact that we see the electron guarantees that it is set in motion; even if it was at rest before we saw it, it can not be so afterwards. The mere carrying out of the experiment spoils the result aimed at. Notice that if we are content with knowing the position rather inaccurately we need not use light of a very short wave-length, and shall not then get much recoil; but if we want the position accurately, we must have a short wave-length and then the recoil will be large. So we see that the uncertainty principle is maintained; high precision in position or velocity can only be attained by the sacrifice of precision in the other.

We have seen how one method of defrauding the

uncertainty principle is defeated, but may there not be others that are more successful? Of course, the only way of proving that none can succeed is by the use of the general principles of the quantum theory, but it is profitable to consider a few further examples and show in detail how the attempts fail. We have seen that a microscope is no use, and so we try to make use of a method that does not require one. If, for example, we have a shutter with a very small hole in it, and have a source of electrons on one side, then if we find one on the other, we know it must have come from the hole, and so we can locate its position in that way. We must work out the experimental arrangements a little more carefully. The experiment might be done in this way. We have a pair of parallel plates ABC and FGH. Electrons start at rest from ABC and fall under the influence of a force

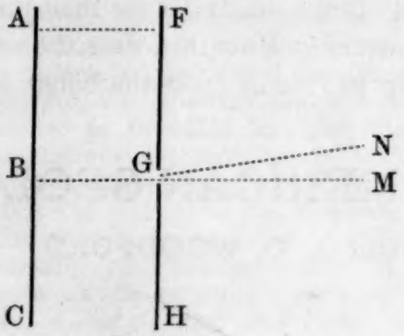

FIG. 1

towards FGH. They will move exactly in directions parallel to AF. The idea behind the experiment is a little more complicated than before because it is necessary to consider the different directions separately. When an electron has emerged from the hole at G, it thereby tells us what its position is as far as concerns the direction GF, but says nothing about its position in the direction GM. So our interest is in the velocity in the direction GF and we do not care what the component in the direction GM may be. Now to the left of G we know the electron's motion to be along BG, that is to say its component in the direction GF is zero. We seem to have conquered the uncertainty principle since we know the speed to be zero and the position is given as accurately as we please by taking the hole at G small enough. But we only know that the electron was on the line BG and not on the line AF because it emerges at G, and in emerging it will be diffracted, say along the line GN, and so will acquire a component of velocity transversely, and one which is uncertain in amount. Once again the mere fact of finding the position has introduced an unwanted velocity. Notice too that if the hole is rather large there is not much diffraction and so very little uncertainty in the velocity, but to counterbalance this advantage there is no very precise knowledge of the position; while, on the other hand, with a very small hole we can fix the position accurately, but pay for it by strong diffraction and so great uncertainty in the speed of the electron after it has emerged.

We will not yet confess defeat. It is true that the electron has been diffracted, but can we not measure through what angle it has turned? If we can do so we can conquer the uncertainty principle, not by avoiding the effect of the observing instrument, but just as successfully by measuring it. We might proceed for example as follows. The electron has altered its course in passing through G. A force of some kind must be necessary to produce this deflection, and this will react on the shutter and tend to push it in the opposite direction. If then we measure this reaction we can assert what path the electron has taken, and this is what we want to know. The simplest way of observing the reaction is to make the shutter free and very light, so that as the electron passes it will be set in motion. We adopt this method. But if the shutter is free, how do we know where the slit is at the moment the electron is passing? We have settled the question of the momentum satisfactorily, but in doing so have lost the position. We must try again, and devise a plan by which to know the position. We therefore send a beam of light through the hole and by watching this beam we can see where the hole is. Surely we now know both position and momentum at the same time. But no, we have forgotten something, for the light itself will behave in the same way as it did in the microscope; it will be diffracted at the hole and will itself start giving impulses to the shutter. There is no way of knowing whether the impulse we observe belongs to the electron or to the light, so that we have regained the measurement of the position, but have paid the price by once more losing the momentum.

It is not by any means easy always to detect the fallacy in experiments like this, but there always is something wrong. Each time we find the defect in our process, we must install some extra piece of apparatus to put it right, and the addition, in the course of overcoming the old difficulty, always introduces a new one. There is no escape from the uncertainty principle.

The uncertainty principle is essentially only concerned with the future; we can install instruments which will tell us as much about the past as we like. Suppose, for example, that we have two shutters, each provided with a very small hole, and a source of electrons to the left of both. The holes are usually blocked up, but for a very short space of time I first open the one in the left shutter, and at a definite time later I do the same for the one on the right. I look for electrons to the right of both shutters. If I see one, I can be quite certain that it went along the line between the holes and took a definite time in doing so; that is to say, I can know its position and speed

precisely. What the principle asserts is that this knowledge is no use in predicting what is going to happen later, for it gives no knowledge of how the electron will be diffracted on emerging from the second hole.

This must revolutionize our ideas about one of the most fundamental principles which have always been accepted in science, the principle of causality. We are accustomed to take it for granted that a *full* knowledge of the present would enable us confidently to predict the future. When we are defeated in our attempts at prophecy, we attribute it to ignorance, with the tacit assumption that with more knowledge of the present we could have done better. It never occurred to any one that the present is definitely unknowable; but we have just seen that the mere effort to know it can not help introducing new errors in the determination. It has been suggested that the new outlook will remove the well-known philosophical conflict between the doctrines of free will and determinism, and it has been welcomed by many for that reason. I would personally offer a most strenuous opposition to any such idea. The question is a philosophic one outside the region of thought of physics and I can not see that physical theory provides any new loophole. We can not say exactly what will happen to a single electron, but we can confidently estimate the probabilities. If an experiment is carried out with a thousand electrons, what was a probability for one becomes nearly a certainty; that is to say, we shall expect to have to repeat our experiment a great many times before we get a result departing far from the average. Physical theory confidently predicts that the millions of millions of electrons concerned in matter-in-bulk will behave even more regularly, and that to find a case of noticeable departure from the average we should have to wait for a period of time quite fantastically longer than the estimated age of the universe. How then does the uncertainty principle help to free us from the bonds of determinism?

SERIAL LITERATURE USED BY AMERICAN GEOLOGISTS

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In 1927, Gross and Gross[1] applied the method of statistical investigation in its simplest form to the problem of evaluation of the periodical literature of a science. They tabulated the references, to other periodicals, in a volume of the *Journal of the American Chemical Society,* and drew certain conclusions concerning the needs of college chemistry libraries. They expressed the hope that workers in other fields might make similar surveys. The interest among librarians and chemists was sufficient to show that the results were worth the labor expended. More recently, other studies have appeared, dealing with mathematics[2] and electrical engineering.[3]

A primary difficulty was encountered in sciences other than chemistry. The *Journal of the American Chemical Society* seems to be unique among scientific periodicals, in that a single volume contains more than 5,000 pages, about 700 articles, and about 5,000 citations to serial literature. It is also sufficiently well balanced in regard to the various branches of chemistry to assure a representative sample of the needs of the American chemist. In other sciences several source journals must be selected.[4]

The present investigation deals with the serial literature of geology, including mineralogy. Six American journals for 1929 were chosen, and the references tabulated. In Table I are listed these *source journals,* together with the total number of pages of the actual articles studied, the total number of citations in each journal, the number of references to books and to personal communications, and the net total, which represents the citations to serial literature. It is these last mentioned references which will be considered in further detail. The totals are probably slightly high, due to unintentional counting in single articles of repetitions of the same citation.

The net total of 3,574 references from six journals of geology (Table I) corresponds to a total of 2,165 from nine journals of mathematics, as reported by Allen,[5] and about 5,000 such references from a single volume of the *Journal of the American Chemical Society.* The contrast between chemistry and the other sciences is evident.

The count of references to books and to personal communications was made because it shows the relative importance of the various sources of information.

If one considers several source journals to be of equal importance, it is evident that there are at least three distinct methods of evaluation: first, an equal

[1] P. L. K. Gross and E. M. Gross, "College Libraries and Chemical Education," SCIENCE, 66: 385, 1927.

[2] Edward S. Allen, "Periodicals for Mathematicians," SCIENCE, 70: 592, 1929.

[3] J. K. McNeely and C. D. Crosno, "Periodicals for Electrical Engineers," SCIENCE, 72: 81, 1930.

[4] An investigation of the serial literature of physics, in progress here, suggests that the *Physical Review* is now so large and varied that it may prove adequate as a single source journal for the science.

[5] *Loc. cit.*

TABLE I

Name of source journal	Number of pages	Total number of references	References to books	Personal communications	Net total
*Am. J. Sci.**	860	898	91	33	774
Am. Mineral.	401	505	53	6	446
Bull. Am. Ass. Petroleum Geol.	959	588	36	33	519
Bull. Geol. Soc. Am.	528	916	158	11	747
Econ. Geol.	787	683	87	15	581
J. Geol.	746	594	57	30	507
Totals	4,281	4,184	482	128	3,574

* Insofar as possible the abbreviations used in this paper follow *Chemical Abstracts,* and may be found in its list of periodicals abstracted, published October 20, 1926, as Part 2, Vol. 20, No. 20. For the journals mentioned here which are not to be found in this list, the writers have chosen abbreviations the meaning of which, it is hoped, will be obvious. The usage of the *U. S. Geological Survey Bulletin* No. 746 (1922) is not followed because of the frequent use of initials, *e.g., J. G.* for *Journal of Geology.* Incidentally the writers desire to protest against the common use in references of mere initials, such as *A.A.A.G., A.A.A.S.,* and *A.A.P.G.* Still less satisfactory are partial or complete translations of foreign titles, such as *Proc. kön. Akad. Wettenschappen Amsterdam* or *Proc. Inter. Geol. Congress.* One common abbreviation, *Min. Mag.,* is ambiguous.

time period may be considered; second, an equal net total of references may be used; third, an equal number of pages of each source journal may be studied. After careful consideration of these three methods, the first was chosen, and complete volumes for 1929 were used with one major and two minor exceptions. These exceptions follow: (1) But eight numbers (May to December, both inclusive) of the *Bulletin of the American Association of Petroleum Geologists* were used. This was, in part, due to the fact that a single volume of this journal contains many more pages than any other source journal considered. (2) The first 1930 issue of the *Journal of Geology* was used instead of the Chamberlin memorial number of 1929, which was more nearly biographical than geological in character. (3) A few articles in the *American Journal of Science* were not included because they dealt with mathematics or pure chemistry. After the main study was completed an investigation was made (by means of weighting) of the other two methods and but few changes, all of little importance, were found. This was to be expected, as inspection of Table I will show that there are few large deviations from equal numbers of references or equal numbers of pages in the source journals as they were used.

The choice of source journals was limited to American publications in order to show the serial literature actually used by American geologists. Excepting paleontology, the various geological sciences are more or less adequately represented. Some paleontological papers are present in the source journals used, but the synonymies of taxonomic descriptions are not included. The *Journal of Paleontology* was not used as a source because its 1929 citations do not begin to cover the varied library requirements of American paleontologists.

Table II*a* shows the results of the tabulation of the citations to the twenty-two journals most frequently mentioned. The distribution of references is

TABLE II*a*

	Name of journal cited	Net total citations	1925–1929	1920–1924	1915–1919	1910–1914	1905–1909	1900–1904	1832–1899	No. of libraries*
1	*Am. J. Sci.*	240	59	40	39	36	18	12	36	97
2	*U. S. Geol. Surv. Bull.*	234	26	60	40	42	38	13	15	......
3	*J. Geol.*	207	62	41	29	25	23	20	7	......
4	*Bull. Geol. Soc. Am.*	187	65	51	18	15	10	16	12	77
5	*U. S. Geol. Surv. Prof. Papers*	144	31	28	30	21	23	11	......	......
6	*Econ. Geol.*	143	64	40	21	11	7	......	......	......
7	*Bull. Am. Ass. Petroleum Geol.*	142	119	20	3	......	......	......	......	44
8	*Z. Krist.*	76	17	23	6	5	4	7	14	27
9	*Am. Mineral.*	65	49	15	1	......	......	......	......	49
10	*U. S. Geol. Surv. Water Supply Papers*	51	10	4	4	10	18	4	1	......
11	*Science*	47	17	1	3	8	15	1	2	115
12	*Can. Geol. Surv. Mem.*	46	13	8	14	10	1	......	......	......
13	*Neues Jahrb. Mineral. Geol.*	46	16	5	5	5	4	4	7	39
14	*U. S. Geol. Surv. Geol. Atlas*	45	4	4	1	3	17	7	9	......
15	*Trans. Am. Inst. Min. Met. Eng.*	41	13	6	7	4	1	3	7	56
16	*Compt. rend.*	40	3	6	11	4	3	2	11	80
17	*Quart. J. Geol. Soc.*	39	11	1	0	3	1	0	23	59
18	*U. S. Geol. Surv. Monographs*	37	0	0	2	8	1	6	20	......
19	*Eng. Min. J.*	34	22	5	0	4	1	1	1	76
20	*J. Wash. Acad. Sci.*	33	16	6	9	2	......	......	......	58
21	*U. S. Geol. Surv. Ann. Repts.*	29	......	......	......	......	......	11	18	......
22	*Geol. Mag.*	28	5	3	2	3	5	7	3	56

* The data in this column were obtained from the "Union List of Serials" (1,588 pages, H. W. Wilson and Company, New York, 1927), and give the number of libraries which reported current accession of the serial indicated. The "Union List" does not include government publications, and does not give exact data for some common journals. Two hundred and twenty-five general and special libraries contributed information.

by five-year periods. It appears that the leading serials hold their own whether long or short periods are considered. The large percentages of the references to the *Bulletin of the American Association of Petroleum Geologists* and to the *American Mineralogist* which fall in the last five-year period are indicative of the growing importance of these recently established journals.

The totals for the next thirty-one ranking journals are given in Table II*b*. The writers do not mean to give the impression that the dividing point between Tables II*a* and II*b* has significance. They feel, however, that the distribution into short time periods is without meaning when but few citations occur. Toward the end of Table II*b* will be found journals with so few references that their inclusion on a strict statistical basis would not be justified.

TABLE II*b*

Name of journal cited	Number of citations	Number of libraries*
N. Y. State Mus. Bull.	27	66
Z. Gletscherkunde	26	20
Geol. Fören. Förh. (Stockholm)	25	18
Univ. Calif. Pub. Bull. Dept. Geol. Sci.	25	52
Univ. Tex. Bull.	24	....
Carnegie Inst. Pub.	23	....
J. Am. Chem. Soc.	23	102
Centr. Mineral. Geol.	21	29
Am. Geol.	20	....
Smithsonian Inst., Miscellaneous Collections	20	....
Petermanns Mitt.	19	39
Z. deut. Geol. Ges.	19	29
Can. Geol. Surv. Summary Rept.	18	....
Geogr. Rev.	18	....
Z. prakt. Geol.	18	34
Bull. comm. géol. Finlande	17	....
Mineral. Mag.	17	32
Ontario Bur. Mines Bull.	17	....
Pan-American Geol.	17	38
Skrifter Norske Videnskaps-Akad. Oslo	17	22
Bull. Mus. Comp. Zool., Harvard	16	34
Geogr. J.	16	....
Bull. soc. franc. minéral.	15	24
Geografiska Ann. (Stockholm)	15	12
Okla. Geol. Surv. Bull.	15	....
J. Chem. Soc.	14	80
Oil and Gas J.	14	29
Proc. Nat. Acad. Sci.	14	....
Trans. Roy. Soc. Can.	14	74
Z. anorg. Chem.	13	63
Z. Ges. Erdkunde (Berlin)	13	24

* See note to Table II*a*.

Many well-known general and geological serials are missing from Tables II*a* and II*b*. Since low standings might be peculiar to the 1929 files, citations to certain of these publications were sought in the 1927 and 1928 volumes of the source journals, with the results shown in Table III.

TABLE III

Name of journal	Citations in 1929	Citations in 1928	Citations in 1927	Number of libraries*
Bull. soc. géol. France	9	5	5	33
Bull. Seism. Soc. Am.	4	7	2	49
Bull. Geol. Inst. Univ. Upsala	5	7	7	34
Compt. rend. Congr. géol. intern.	9	1	8	....
Geol. Zentr. (abstract journal)	8	1	1	40
Geol. Rundschau	5	1	5	21
Jahrb. preuss. geol. Landesanst.	8	1	2	9
Jahrb. geol. Bundesanst., Wien	2	1	3	20
Proc. Roy. Soc. London, (A)	6	6	5	70
Sitzb. preuss. Akad. Wiss., phys.-math. Kl.	9	5	3	33
Sitzb. Akad. Wiss., Wien	8	0	11	25
Trans. Roy. Soc. Edinburgh	5	11	7	47
Tschermaks mineral. petrog. Mitt.	11	7	5	23
Z. Vulkanologie	6	0	2	24

* See note, Table II*a*.

In no case is there a rise to the horizon of Table II*b*.

While the data for Table III were being obtained, it was noticed that most of the serials of Table II*b* were often mentioned in 1927 and 1928. The exceptions noted were the *Zeitschrift für Gletscherkunde* and the *Geografiska Annaler*. The high positions of these two journals in 1929 were due to a large number of glacial papers published in our source journals during that year.

The last column of Table III indicates that the serials of this table are nearly as common in American libraries as are the series listed in Tables II*a* and II*b*. In fact, half the journals in Table III have a wider distribution among the libraries considered than does the *Zeitschrift für Kristallographie,* the first foreign serial of Table II*a*. In this connection, it may be suggested that the importance of a given file for geologists is not directly proportional to the number of libraries possessing it. On the contrary, it may be argued that the inverse is more nearly true for journals to which there are approximately equal numbers of citations. For instance, by this test the *Geologiska Föreningens i Stockholm Förhandlingar* are more important than the *University of California Publications Bulletin of the Department of Geological Sciences,* as the position of the former was attained

in spite of relative inaccessibility to American workers (see Table II*b*).

Table IV summarizes the data concerning the frequency of references to the various serials. The 480 publications to which citations occurred were found to be distributed among thirty countries (counting British dominions and colonies) and fifteen different languages.

TABLE IV

	No. of serials	No. of references	Per cent. of references
Serials included in Table II*a*	22	1,954	54.7
Serials included in Table II*b*	31	570	15.9
Serials cited 7 to 12 times	36	303	8.5
Serials cited 4 to 6 times	52	246	6.9
Serials cited 3 times	48	144	4.0
Serials cited 2 times	66	132	3.7
Serials cited 1 time	225	225	6.3
Totals	480	3,574	100.0

In choosing source journals the writers limited themselves to national American periodicals. As one test of the selections Table V shows the effect of excluding from the citations to the source journals all self-references, that is, references to papers published in the same journal.

TABLE V

Name of source journal	Table II*a*, totals	Self-references	Other references
Am. J. Sci.	240	94	146
J. Geol.	207	70	137
Bull. Geol. Soc. Am.	187	59	128
Econ. Geol.	143	115	28
Bull. Am. Ass. Petroleum Geol.	142	118	24
Am. Mineral.	65	44	21

It will be seen from the last column of Table V that there is no change in the relative rank of the source journals. There does appear, however, a striking difference between the first three journals and the second three. Although other explanations are possible, it is believed that this is due to the unique positions of the latter in specialized fields. Such an explanation strengthens the case for including the last three serials as source journals.

Government publications have an important place in geological literature. This is evident from the data

TABLE VI

Country	References to governmental publications			Other references
	National	Provincial and state*	Total	
United States	607	299	906	1,504**
Canada	97	19	116	23
Gt. Britain and poss. (except Canada)	6	11	17	244
Germany and Aus.	2	17	19	448
France	1	1	2	71

* Including state universities.
** Of these, 984 are to the six source journals.

of Tables II*a* and II*b*. Fifteen per cent. of the total of 3,574 references are to the various series of the United States Geological Survey. It should prove interesting, therefore, to compare the relative importance to American geologists of their own and other governmental publications. These data are summarized in Table VI and show at once three things: first, United States and Canadian governmental reports are of prime importance; second, the small number of Canadian "Other references" suggests that the journals of the United States serve both this country and Canada; third, foreign governmental publications are but rarely used by American geologists.

Many of the domestic references are to areal surveys, and the corresponding publications of other governments are vastly less essential to the North American worker. However, the rarity of references to foreign surveys suggested an analysis of the total number of foreign references, to determine in what portions of the field of geology American workers look abroad. To this end, the citations in individual source journals were tabulated. The differences between the sources were so striking that the writers decided to include the data for 1928 and part of 1930 (for this tabulation only), as checks upon 1929. These results are shown in Table VII.

It appears that journals which include mineralogical papers contain more foreign references than do the more strictly geological periodicals. Further analysis is difficult, and differences seem to be largely idiosyncrasies of individual authors. An example of this variability is found in the 1929 *Journal of Geology*, where all of the references to foreign books or periodicals occur in 17 of the 50 papers. However, glacial geologists seem rather consistently to use foreign data, as shown by the frequent European references in four of the seven glacial papers in this volume of the *Journal of Geology*, and also by the obviously elevating effect of a glacial symposium upon the foreign percentage for 1929 of the *Bulletin of the Geological Society of America* (Table VII).

TABLE VII

Name of source journal	Per cent. of references to serials published outside the United States and Canada			
	1928	1929	1930*	1928–1930
Am. J. Sci.	31.1	30.5	29.3	30.2
Am. Mineral.	36.0	55.6	35.8	42.7
Bull. Am. Ass. Petroleum Geol.	18.1	10.6	10.5	12.2
Bull. Geol. Soc. Am.	17.1	40.0	19.3	28.4
Econ. Geol	23.9	17.4	18.4	20.0
J. Geol.	14.8	10.9	9.9	12.0

* All 1930 issues of the source journals available here on November 24, 1930, were considered, *i.e.*, *Am. J. Sci.*, January to November; *Am. Mineral.*, Nos. 1–11; *Bull. Am. Ass. Petroleum Geol.*, Nos. 1–11; *Bull. Geol. Soc. Am.*, Nos. 1–2; *Econ. Geol.*, Nos. 1–7 and Suppl. to No. 3; *J. Geol.*, Nos. 1–7.

Finally, Table VIII presents the 1,015 foreign references (Canada again considered as domestic, as justified above) classified according to language. Danish, Norwegian and Swedish are grouped together as Scandinavian. The great relative importance of German is apparent. French suffers, perhaps, from the scarcity of paleontological references in the sources

TABLE VIII

Language	Foreign references	
	Number	Per cent.
German	486	47.9
English	262	25.8
Scandinavian	87	8.6
French	84	8.3
All others	96	9.4
Totals	1,015	100.0

considered, but probably gains in number of titles because of the brevity of the contributions (forty in number) to the principal French periodical, *Comptes rendus.*

OBITUARY

MEMORIALS

THE significance of the work of John Bartram was commemorated by representatives of leading botanic and horticultural associations in this country and in England at the celebration of the two hundredth anniversary of the founding of Bartram's Garden, the first botanic garden in the American colonies. The observance was held at the Academy of Natural Sciences in Philadelphia on June 5 and 6, also at the Bartram Garden, overlooking the Schuylkill River, by the John Bartram Association, the American Philosophical Society, the Pennsylvania Horticultural Society and the Academy of Natural Sciences. Among the speakers were Dr. Rodney Howard True, professor of botany at the University of Pennsylvania, and Dr. John Hendley Barnhart, bibliographer of the New York Botanic Garden. An address by Dr. Witmer Stone, vice-president of the Academy of Natural Sciences of Philadelphia, was read in his absence.

The British Medical Journal reports that a fund is being raised to establish a permanent memorial to Dr. Hughlings Jackson. Among those who are taking action in the matter are many old friends and pupils who revered and loved Jackson, and who recognize to what an extent the preeminent position of British neurology in the medical world is due to his work and influence. He was among the great leaders of modern neurology, and it is much to be desired that the inspiration that he gave to so many in his lifetime should be kept fresh, and still serve as a stimulus to a younger generation who knew him not. It is hoped to raise an amount sufficient to provide a permanent endowment for the Hughlings Jackson Lecture, given every third year before the section of neurology of the Royal Society of Medicine. Several generous promises of support to such a fund have already been received, but the signatories of this letter feel that an opportunity to subscribe should be given to many who can only be reached through the publicity of the press, and who would certainly wish to show their appreciation of the position which Hughlings Jackson holds in the history of modern medicine. Dr. Wilfred Harris, of 56, Wimpole Street, London, W.1, has consented to act as treasurer of the fund, and subscriptions should be sent to him, marked "Hughlings Jackson Memorial Fund."

RECENT DEATHS

DR. FRANKLIN HENRY GIDDINGS, professor emeritus of sociology at Columbia University, died on June 11. He was seventy-six years old.

DR. JOSEPH H. HATHAWAY, assistant professor of anatomy at the University of Michigan, died on June 12, at the age of fifty-two years.

MISS EMILY HOWSON, professor of astronomy at Agnes Scott College, Decatur, Illinois, died on June 6th.

THE death is announced of M. Raoul Gautier, honorary director of the Geneva Observatory.

PROFESSOR G. B. FROSTERUS, director of the Institute for Soil Science, Helsinki, died on March 1 at the age of sixty-five years. Dr. Frosterus took part in the development of soil science in Finland and was an active member of the International Society of Soil Science.

THE death at the age of ninety-two years is announced of Professor Welhelm Franz Exner. Dr. Exner was for some time professor at the College of Agriculture of the University of Vienna and later professor of mechanical technology and engineering.

Nature reports the death of Dr. Rudolf Marloth, who was president of the South African Association for the Advancement of Science in 1914 and author of works on the flora of South Africa, and of Dr. Alwin Berger, an authority on succulent plants and cacti, who contributed a monograph on the Crassulaceae to Engler-Prantl's "Natürliche Pflanzenfamilien."

SCIENTIFIC EVENTS

BOTANICAL RESEARCH STATIONS IN AFRICA

SIR ARTHUR HILL, director of the Royal Botanic Gardens, Kew, recently addressed the Dominions and Colonies Section of the Royal Society of Arts on the scientific research work he had seen during his recent tour in South and East Africa.

According to an abstract in the London *Times* the lecturer described the principal centers of research work that he visited and singled out the National Botanic Garden at Kirstenbosch as one of the most remarkable. Here, he said, with the unique and magnificent setting of Table Mountain and the groves of the beautiful Silver Tree (*Leucadendron argenteum*), there was being built up a garden which, with proper care and attention in the way of sufficient funds for maintenance and development, should be one of the great botanic gardens of the world. Kirstenbosch was bought by Cecil Rhodes, in 1895, as part of his far-sighted scheme for preserving the eastern slopes of Table Mountain and Devil's Peak as a National Park, and in 1913, thanks to the efforts of the late Professor Harold Pearson and Sir Lionel Phillips, a portion of the estate was set aside by government for the establishment of a National Botanic Garden.

It was very much to be hoped that no pains or money would be spared in order to carry out to the full the vision of those two benefactors to botany in South Africa, so that the garden might be fully developed; also that the slopes of the mountain might be adequately preserved both from the depredations of forest fires and from the incursions of exotic trees. Thus only could we hope to see Rhodes's vision of a great National Park on Table Mountain and Pearson's conception of a South African National Garden properly honored by memorials of supreme interest and value to the whole world.

Referring to the East African Agricultural Research Station at Amani, in the East Usambara Mountains, Tanganyika Territory, Sir Arthur said that the question of soils was also one of great importance to all the East African Territories and a Soil Museum was being built up at Amani, which in course of time should be as useful as a herbarium with its botanical specimens. Those soil samples would be of particular value in ascertaining the physical and chemical properties of those East African soils known to be subject to serious erosion, which was so important a problem in the tropics.

THE REFORESTATION PROGRAM

FOREST planting by all agencies in the United States amounted last year to 138,970 acres, a gain of 24 per cent. over 1929, according to completed reports from 43 states and territories made public on June 6 by the Forest Service of the Department of Agriculture. Last year's planting brought the cumulative record for all lands reforested to date in the United States to 1,798,048 acres. Federal, state, municipal and private plantings all made substantial gains despite drought and adverse economic conditions.

Other than the federal and state governments, 19,161 agencies and individuals participated in forest planting last year, which set the new record for acreage reforested. Of the more than 17,000 individuals about four fifths were farmers.

Forest Service plantings in the National Forests amounted to 21,678 acres, 19 per cent. more than the year preceding. Forest Service plantings are planned on a still larger scale this year, and spring planting has been active in several National Forests.

State forestry department plantings last year amounted to 41,038 acres, a gain of 30 per cent. over 1929. Plantings by municipalities aggregated 9,214 acres, an increase of 55 per cent. Industrial organizations planted 30,230 acres, a gain of 20 per cent., and organizations of other types, with 2,518 acres planted, gained 66 per cent. Schools and colleges put out 825 acres of trees, 53 per cent. more than the

year before. Individual plantings jumped from 28,475 to 33,467 acres, a gain of 17 per cent.

Last year's totals showed important progress and interest in renewing forest resources and putting idle lands to growing timber crops, although planting has never yet kept pace with losses through wasteful cutting, forest fires and erosion.

Michigan led all the states in acreage reforested for 1930, with a grand total of 38,302 acres planted by all public and private agencies. Of this area, the Forest Service planted 8,452 acres and the state 26,617 acres.

Forest planting in New York by various state, municipal and private agencies reached 24,250 acres. Pennsylvania planted 18,048 acres to public and private forest.

Planting in Delaware, Maryland and New Jersey aggregated 1,672 acres. New England reports show a total of 11,614 acres planted. The South Atlantic States planted 5,556 acres, Georgia leading with 2,542. Gulf States set out 7,869 acres, Louisiana's share being 6,556.

In Ohio, private and public agencies planted 2,633 acres, largely farm woodlands. Beginnings were made in several Central States with reforestation used especially as a check to erosion of farm lands. Part of Wisconsin's plantings of 6,086 acres were for watershed and farm.

THE GUATEMALA EXPEDITION

DESPITE a severe rainy season, tangled jungle trails and the illness of one of the party, the University of Michigan expedition into the interior of Guatemala has returned with an unusually large number of important specimens. Members of the party included Professor Harley H. Bartlett, botanist; Dr. Josselyn Van Tyne, ornithologist, and Dr. Adolph Murie, mammalogist, they having undertaken the biological phase of a broad survey by the Carnegie Institution.

Meeting with Carnegie archeologists at Belize, British Honduras, on January 26, the party planned to proceed at once to the old Maya city of Uaxactun, but were turned back by news that heavy winter rains had made jungle trails impassable. While waiting for the trails to become passable, the "Pine Ridge" area was visited. This involved a trip of three days and nights of travel in small boats up a shallow winding river. Sharply demarked from surrounding jungle, this "pine ridge" appears much as if a strip of northern Michigan's open pine woods had been transplanted in the tropics.

Finally the jungle trails were reported "passable for mules," and the party returned to their base at El Cayo. But "passable for mules" proved almost impassable for men, and the sixty-five miles inland required four days of the hardest kind of travel and the simple "bush" camp at Uaxactun looked luxurious when finally reached.

In these jungles 1,900 years ago the Mayas began the building of their great stone cities. They have now become a tangled jungle almost unknown to white men and specimens, exceptional both in quantity and in quality, were secured.

Due to the large amount of material and limited accommodations, the party broke up, Professor Bartlett going out first with his extensive botanical collection. On arriving at El Cayo, the mule train was to unload and return for Drs. Van Tyne and Murie, before the rains should set in making travel impossible. Unfortunately at this point Dr. Van Tyne was taken with a sudden and severe attack of jungle fever. Dr. Murie, however, and Mr. Monroe Amsden, of the Carnegie party, finally brought him and all the collections safely back to civilization.

The classification of the specimens will be carried out at the museum. It is probable that a second visit to this region will be made next year.

APPROPRIATIONS FOR GRANTS-IN-AID BY THE NATIONAL RESEARCH COUNCIL

AT its meeting in May the National Research Council's Committee on Grants-in-Aid made grants for the support of research as follows:

To S. J. Barnett, professor of physics, University of California at Los Angeles, magnetization by rotary fields; Harry E. Farnsworth, associate professor of physics, Brown University, electron diffraction and refraction by metal crystals; R. C. Gibbs, chairman of the committee on ruled gratings of the American Physical Society, professor of physics, Cornell University, improvement of facilities for the manufacture of diffraction gratings; Ernest O. Lawrence, professor of physics, University of California at Berkeley, the production of high velocity hydrogen ions without the use of high voltages; Arthur E. Ruark, professor of physics, University of Pittsburgh, measurement of wave-lengths and line-widths in the spectra of Gamma rays; Karl S. Van Dyke, professor of physics, Wesleyan University, the piezo-electric effect in quartz and Rochelle salt

Wilber E. Harvey, instructor, Lehigh University, the combined effects of corrosion and fatigue upon welds.

Frank T. Gucker, Jr., assistant professor of chemistry, Northwestern University, the thermo-chemistry of solutions and the dielectric constant of the solvent.

M. R. Campbell, principal geologist, U. S. Geological Survey, the gravel deposits of the Piedmont Plateau and Atlantic coastal plain north of Virginia; C. H. Crickmay, assistant professor of geology, University of Illinois, the Jurassic deposits of Mt. Jura, California; Richard M. Field, associate professor of geology, Princeton University, the stability of the Bahama

Islands in relation to their origin, migration and alteration of the sediments which mantle their surfaces; Edwin T. Hodge, professor of geology, University of Oregon, completion of geological study of Mt. Hood region in Oregon; Frank O. Melton, associate professor of geology, University of Oklahoma, tectonics of the continental interior of North America in relation to the Appalachian orogeny; Ellen C. Semple, Clark University, preparation for publication of "Geographic Influences in the History of the Mediterranean Region."

T. Hume Bissonnette, professor of biology, Trinity College, modification and control of the sexual cycle in the European starling; George O. Burr, associate professor of botany, University of Minnesota, the rôle of fatty acids in animal metabolism; Cleveland S. Simkins, associate professor of anatomy and embryology, University of Tennessee Medical School, the human ovary from birth to sexual maturity.

Ralph E. Cleland, associate professor of biology, Goucher College, cytological and gentical studies of *Oenothera;* George R. La Rue, professor of zoology, University of Michigan, distribution of the lung fluke, *Paragonimus,* in America; Charles L. Parmenter, associate professor of zoology, University of Pennsylvania, chromosome formation in parthenogenetically produced frogs; James T. Penney, associate professor of biology, University of South Carolina, cell behavior in fresh-water sponges; William Rowan, associate professor of zoology, University of Alberta, bird migration from the view-point of animal behavior; Carl G. Vinson, professor of horticulture, University of Missouri, the virus diseases of plants.

Franklin Fearing, associate professor of psychology, Northwestern University, the functions of the non-acoustic portion of the labyrinth in pigeons; Frederick S. Hulse, research assistant in anthropology, Bishop Museum, Honolulu (at present at the Peabody Museum, Harvard University), race mixture between Spanish, Indian and Negro stocks; John A. McGeoch, professor of psychology, University of Missouri, the influence of the time interval and of the point of interpolation upon degree of retroactive inhibition; Jessie W. Murray, acting director, Tioga Point Museum, investigation of aboriginal Indian sites near Athens, Pennsylvania.

Vernon Kellogg
Permanent Secretary,
National Research Council

SCIENTIFIC NOTES AND NEWS

Dr. Leland O. Howard, until his retirement in 1927 chief of the Bureau of Entomology of the U. S. Department of Agriculture, Washington, D. C., has been awarded the 1931 Capper Gold Medal and the sum of $5,000 for distinguished service to American agriculture. Last year's award was given to Professor Stephen Moulton Babcock, professor emeritus of agricultural chemistry at the University of Wisconsin.

The University of Paris conferred on June 13 honorary degrees on Dr. Henry Fairfield Osborn, director of the American Museum of Natural History, New York, and on Dr. Walter B. Cannon, professor of physiology in the Harvard Medical School.

The honorary doctorate of science was conferred on Dr. Howard McClenahan, secretary of the Franklin Institute, formerly professor of physics and dean of the college at Princeton University, at the recent commencement exercises of the University of Pennsylvania.

Dr. Karl Landsteiner, a member of the Rockefeller Institute for Medical Research, New York, was recently elected a member of the Royal Danish Academy of Sciences in Copenhagen, in the class of the natural sciences and mathematics.

Mme. Curie was recently appointed an honorary member of the Sociedad Española de Fisica y Quimica in a ceremony at the University of Madrid, where she delivered lectures on radioactivity. Mme. Curie went to Spain on the invitation of several national medical and scientific societies.

Dr. Hans Horst, professor of pharmacology at the University of Vienna, has been elected a foreign member of the Royal Swedish Academy of Sciences.

Sir Oliver Lodge celebrated his eightieth birthday on June 12.

The American College of Radiology on June 10 awarded its gold medal to Dr. Charles C. Lauritsen, professor of physics at the California Institute of Technology, Pasadena, in recognition of his work resulting in "the first practical high voltage tube operating daily for experimental research."

M. Paul Pelseneer was recently elected a correspondent of the Paris Academy of Sciences in the section of anatomy and zoology in the place of the late M. A. Brachet.

At the Philadelphia meeting of the American Medical Association gold medals were awarded to Dr. Jacob Furth, of the Henry Phipps Institute of the University of Pennsylvania, for experiments demonstrating that leukemia can be transmitted by a filterable virus, and to Drs. J. Parsons Schaeffer and Warren B. Davis, of Jefferson Medical College, Philadelphia, for anatomical researches on the nasal sinuses. Silver medals were awarded to Drs. Harrison S. Martland, A. V. St. George, Alexander O.

Gettler and Ralph H. Mueller for their detailed presentation of the subject of radium poisoning and to Dr. Bedford Shelmire, of Baylor University School of Medicine, and Dr. W. E. Dove, of the U. S. Bureau of Entomology, for original work on the spread of typhus fever by rat mite.

THE honorary degree of doctor of science was awarded to Admiral Richard E. Byrd and Professor Laurence McKinley Gould, of the University of Michigan, leaders of the Byrd Antarctic Expedition, by the Polytechnic Institute of Brooklyn at the seventy-sixth annual commencement exercises on June 17. Professor Gould delivered the commencement address on "Antarctic Research and the Byrd Expedition." Scientific exploration as indicated by the Byrd expedition will be the general subject at the dinner of the Corporation of the Polytechnic Institute on the evening of June 22 at the University Club of New York, where explorers will gather in informal discussion of the value of world travel and research. On this occasion a review of earlier Arctic explorations will be presented by Major Anthony Fiala to be followed by a study of the recent Vincent Astor Expedition to the Galapagos Islands, by Dr. Charles H. Townsend, director of the New York City Aquarium. Professor Laurence McKinley Gould, of the University of Michigan, will present a paper on the scientific aspects of the Byrd expedition, while Dr. Walter Granger, curator of fossil mammals at the American Museum of Natural History and scientific head of the expedition to Mongolia of the museum, will present a review of the results of that exploration.

THE prize of $250 offered by the Scientific Apparatus Makers of America for the best paper on instruments appearing in *The Review of Scientific Instruments* during the calendar year 1930 has been awarded to Mr. K. C. D. Hickman and Mr. C. R. Sanford, of the Eastman Kodak Research Laboratory, Rochester, for their joint paper entitled "A Study in Condensation Pumps," which appeared in the March issue of the *Review*. The committee made special mention of the paper by Mr. J. D. Hardy entitled "A Theoretical and Experimental Study of the Resonance Radiometer," which appeared in August and also of the paper by Mr. P. H. Carr entitled "A New Method of Recording Electrons," which appeared in December. The committee of award consisted of Professor J. R. Collins, Cornell University, *chairman;* Professor H. W. Webb, Columbia University, and Dr. H. W. Russell, of the Battelle Memorial Institute.

AT the Massachusetts Institute of Technology, Professor Erwin H. Schell has been appointed head of the department of business and engineering administration, and Professor Charles F. Taylor head of the department of aeronautical engineering. Dr. Louis J. Bircher, of Vanderbilt University, will be visiting professor of chemistry.

DR. PAUL KIRKPATRICK, professor of physics at the University of Hawaii, will be acting associate professor of physics at Stanford University during the academic year 1931–32. His place at the University of Hawaii will be taken by Dr. Harry Kirkpatrick, who, with Dr. J. W. M. Dumond, recently completed an investigation of the scattering of x-rays at the California Institute of Technology. Dr. Willard H. Eller will be in charge of the department of physics at the University of Hawaii while Dr. Kirkpatrick is on leave.

DR. A. E. NAISCH, lecturer in medicine at the University of Sheffield, has been promoted to the chair of medicine.

DR. AND MRS. LEWELLYS F. BARKER, of Baltimore, leave in June to spend the summer in Switzerland.

PROFESSOR AND MRS. COCKERELL, of the University of Colorado, sail on June 20 to England, and go thence to Africa, expecting to visit Benguella, Katanga, Lake Tanganyika, Rhodesia, the Cape Province, etc., returning to Colorado at the end of the year. The African expedition will include Miss Alice Mackie and Mr. and Mrs. J. Ogilvie.

DEAN F. B. MUMFORD, of the College of Agriculture of the University of Missouri, has been granted leave of absence until January 1. He plans to spend several months in Europe.

PROFESSOR J. N. LECONTE, of the department of mechanical engineering of the University of California, has been granted a year's sabbatical leave to visit hydraulic laboratories and installations in France and Germany.

THE Medical Fellowship Board of the National Research Council, of which Dr. G. Carl Huber, dean of the Graduate School of the University of Michigan, is the chairman, has made the following appointments of fellows in medicine for the year 1931–1932: Evelyn M. Anderson, Broda O. Barnes, Chandler McC. Brooks, Walter D. Claus, George Lyman Duff, Knox H. Finley, John H. Hanks, Carl M. Johnson, Peter K. Knoefel, Donald McEachern, Robert A. Moore, Harold S. Olcovich and Samuel R. M. Reynolds. The next meeting of the Medical Fellowship Board will be held on September 19, and applications to be considered at that time should be filed on or before August 15.

PROFESSOR WILLIAM E. RITTER, of the University of California, left Berkeley for London on May 23,

where on invitation from the president and executive committee of the Second International Congress of the History of Science and Technology he will participate in the London meeting of the congress from June 29 to July 3. Dr. Ritter will discuss the bearings of the Aristotelian teachings on the historical and contemporary inter-relationship of the physical and biological sciences.

DR. JOHN R. MURLIN, professor of physiology and director of the department of vital economics of the University of Rochester, gave the annual address before the Alpha chapter of Sigma Xi at Cornell University on May 15. His subject was "Modern Aspects of Vitalism."

AT the recent Philadelphia meeting of the American Medical Association, the Billings lecture, named for Dr. Frank Billings, was delivered before the section on medicine by Dr. Henry A. Christian, of the Harvard University Medical School, on the classification of different types of Bright's disease.

DR. HARVEY CUSHING, Moseley professor of surgery at Harvard University, delivered on June 11 the annual discourse at the one hundred and fiftieth anniversary convention of the Massachusetts Medical Society.

PROFESSOR HEBER D. CURTIS, director of the University of Michigan Observatory, gave the address at the dedication on June 3 of the new astronomical observatory at Wittenberg College, Ohio.

THE Ontario Radium Commission is visiting the large cities of the United States, making a study of the methods used in the treatment of cancer. Members of the commission include the Honorable John M. Robb, Minister of Health for Ontario; the Reverend H. J. Cody, chairman of the commission and chairman of the Board of Governors of the University of Ontario; Dr. Herbert L. Lombard, director of the division of adult hygiene, Massachusetts Department of Public Health; Dr. W. T. Connell, professor of medicine at Queen's University, and Dr. J. W. S. McCullough, chief inspector of health for Ontario. On June 26 members of the commission will leave for Europe. They will study facilities for treating cancer in London and other medical centers. Following a tour of Europe they will return to Ontario and will submit a report to the Ontario Legislature.

THE thirteenth annual meeting of the American Society of Mammalogists was held at the Academy of Natural Sciences of Philadelphia from May 12 to 15. Thirty papers on various phases of mammalogy were presented. The annual election of officers resulted as follows: *President,* Marcus W. Lyon, Jr.; *Vice-presidents,* T. S. Palmer, H. E. Anthony; *Recording Secretary,* H. H. Lane; *Corresponding Secretary,* Francis Harper; *Treasurer,* Mrs. Viola S. Snyder; *Directors,* class of 1931–1933, Joseph Grinnell, Remington Kellogg, A. Brazier Howell, W. E. Saunders, Wharton Huber; *Director,* to fill vacancy in class of 1930–1932, Lee R. Dice. Edward A. Preble continues as chairman of the editorial board in charge of the *Journal of Mammalogy.* The total membership of the society is now 1,017. The next annual meeting will be held at the U. S. National Museum in Washington.

A CONFERENCE of Connecticut and Rhode Island investigators in the chemistry and physiology of plants was held on June 5 and 6 in the laboratories of Yale University and of the Connecticut Agricultural Experiment Station. The first session was held at the Experiment Station, where the address of welcome was delivered by Director William L. Slate. The members inspected the experimental work of the various departments of the station, as well as the New Haven Branch of the United States Office of Forest Pathology, and the Marsh Botanical Gardens and Osborn Botanical Laboratory of Yale University. A dinner was given at the Yale Faculty Club and at 8:00 o'clock the conference reconvened in Sage Hall. Professor Andrew Keogh, Yale University librarian, welcomed the members to Yale. Director-Emeritus Russell H. Chittenden spoke on "Plant Sciences in Sheffield Scientific School." Plant science research in the Yale School of Forestry was described by Professor James W. Toumey. Dr. Treat B. Johnson, Sterling professor of chemistry, spoke on "The Need for Cooperation in Biochemical Research." Saturday morning was given to the presentation of some twenty-six research papers at the Osborn Botanical Laboratory.

THE eastern section of the Botanical Society of America will hold its biannual summer meeting at the Pennsylvania State College from June 16 to 19. The program for the four days includes a number of field trips.

THE first permanent exhibition in New York of exact scale models illustrating minutely the development of marine, highway and railroad transportation was opened to the public on May 25 at the Museum of Science and Industry, 220 East Forty-second Street, by Mr. Frederic B. Pratt, president. The exhibits include an arrangement of marine, railroad and automobile engines, and in the highway division there is a series of mechanical sets which may be put in motion to reveal the workings of brakes, axles, springs, gears and spark plugs. The exhibition, a part of a projected series which will ultimately portray the significant steps in the major fields of man's

material evolution, was arranged under the direction of Dr. Charles R. Richards, authority on industrial museums, assisted by Carlos de Zafra, of the engineering faculty of New York University; Charles E. Duryea, co-inventor of the Duryea automobile, and Henry O. Havemeyer, Jr.

THE Rush Rhees Library of the University of Rochester has been given a complete collection of the first editions of all the works of Charles Darwin, including a copy of the first printing of the first edition of the "Origin of Species." It is said that there is no similar collection in the United States.

THE Rockefeller Foundation has granted $30,000 to Iowa State College, to be used for research in biological sciences and related branches of physics and chemistry. The fund is to be paid during the next five years.

ARRANGEMENTS for cooperation between the Hebrew University of Jerusalem and the Field Museum, Chicago, were completed during a visit to the museum by Dr. Julius Magnes, president of the university, on May 8. Dr. Magnes consulted with members of the scientific staff, and formulated plans for exchange of specimens and publications between the two institutions.

LECTURES are announced at the New York Botanical Garden at 4 o'clock in the afternoons as follows: June 6, "John Bartram, American Quaker and Botanist to the King," Dr. John Hendley Barnhart, bibliographer. June 13, "Rice, the Greatest Food Plant in the World," Dr. H. A. Gleason, curator. June 20, "Vegetation of the Philippines," Dr. Elmer D. Merrill, director-in-chief. June 27, "Roses," Mr. Kenneth R. Boynton, head gardener. July 11, "Daylilies," Dr. A. B. Stout, director of laboratories. July 18, "Diatoms, Microscopic Beauties," Dr. Marshall A. Howe, assistant director. July 25, "Edible and Poisonous Mushrooms," Dr. Fred J. Seaver, curator. August 1, "Coal, Its Origin and Development," Dr. Arthur Hollick, paleobotanist. August 8, "New Gladiolus Varieties," Dr. Forman T. McLean, supervisor of public education.

RECOGNIZING the need for developing a body of research minded and research trained individuals specializing in textile and allied fields who might be eventually attracted into the textile industries, the directors of the Textile Foundation have authorized establishment of a fund of $100,000 during the next two years to provide for fellowships in textile research.

WARD'S NATURAL SCIENCE ESTABLISHMENT, which was damaged by fire last September, will be restored and maintained by the University of Rochester. The university is reported to have acted in response to requests from scientific men from all parts of the country. The new museum will be housed in a four-story building containing 40,000 square feet of floor space and will be directed by Dr. Dean L. Gamble.

THE Tennessee House of Representatives on June 11 gave approval to the statute prohibiting the teaching of the theory of evolution in schools wholly or partly supported by state funds by rejecting a bill to repeal the law. There were only fourteen votes against the motion to reject the measure and fifty-eight votes in behalf of the rejection.

IT is reported in the New York *Times* that Greenwich Observatory is to be modernized by the addition of a large new telescope of reflecting pattern, with a mirror thirty-six inches in diameter and with a spectroscope also attached. The cost will be defrayed by William Johnston Yapp, of London, a director of the Cariboo Mining Syndicate, Carreras, Ltd., and Consolidated Gold Alluvials of British Columbia. This telescope will require the addition of a new dome to the observatory, and an order for the telescope and the dome will shortly be placed with the British firm of Howard, Grubb and Parsons Company. A new transit-circle apparatus also is to be constructed to replace that built by Sir George Airy in 1851 which proved, in a judgment expressed twenty-five years ago by the late Simon Newcombe, to be "the most serviceable meridian instrument ever constructed." More than 500,000 observations have been made with this instrument in its eighty years, and it is expected the new one will be built by Cooke, Troughton and Simm, Ltd., of London.

FELLOWSHIPS for advanced training in forestry have been awarded by the Charles Lathrop Pack Forest Education Board, of Washington, D. C., to seven foresters, six American and one Canadian, including college seniors as well as older men of long experience. The fellowships range in value up to $1,500, and the men were selected from about 80 applicants. The fellowships were created to encourage men of unusual intellectual and personal qualities to obtain advanced training that would better qualify them for leadership in some phase of forestry. This is the second award of fellowships by the Charles Lathrop Pack Forest Education Board. The fellowships are available to Americans and Canadians for further training in the general practice of forestry, in the forest industries, in the teaching of forestry, in forest research, or in the development of public forest policy. Applications for the third award will be taken next autumn. Further information can be ob-

tained from the Secretary of the Charles Lathrop Pack Forest Education Board, 1214 Sixteenth Street, N.W., Washington, D. C.

THE annual congress of the Royal Institute of Public Health opened on May 19 in the Aula of the Frankfurt University. Some 200 members and delegates of the institute were present, and among the English visitors were Lord Leverhulme, treasurer, and Professor Sir Thomas Oliver, chairman of the Council of the Institute, and Sir William Smith, a former president. Lord Reading, the president, was unable to attend, and his address was read by the British Consul-General. In the course of the address Lord Reading said that the congress had its own international significance and was engaged in war against a common enemy to defeat and destroy disease and to make for better conditions of life, physical, mental and moral, throughout mankind. In its respective spheres it was traveling, even though it may be subconsciously, in the direction all fervently desired to attain—that of peace and good will among men. At a reception held on Wednesday the Chief Burgomaster of Frankfurt, Dr. Landmann, handed Sir Thomas Oliver, for delivery to the Royal Institute of Public Health, the city's highest decoration—the golden plaque—in memory of the fact that Frankfurt was the first German city to be visited by the institute since the war. Sir Thomas Oliver in return bestowed upon the Chief Burgomaster the honorary membership of the institute for his conspicuous services to the institute.

THE ninth meeting of the International Institute of African Languages and Cultures was recently held in Paris. The congress dealt with important linguistic and anthropological problems of the Africa of to-day. Professor Antoine Meillet, president of the Institut d'Ethnologie, acted as president of the congress, and Professor Henri Labouret as vice-president. The members of the council were received by Dr. Charcot, the president of the Geophysical Society of France, and M. Grandidier, secretary-general of the society. At the beginning of the meeting, which lasted for three days, the chairman announced that the Rockefeller Foundation of New York had decided to give to the institute a yearly contribution of £5,000 for five years, plus a further contribution calculated at the rate of £1 for every £2 obtained by the institute from other sources to enable it to carry out further study and research in Africa.

Nature writes that the Royal Dublin Society will celebrate its bicentenary during June, as it was founded on June 25, 1731, at a meeting held in the rooms of the Philosophical Society in Trinity College, Dublin. The society at its foundation was known as "The Dublin Society for improving Husbandry, Manufactures, and other useful Arts and Sciences," and during the two centuries of its existence its activities have ranged over all the subjects included in the original title, and have been extended to include pure science, the fine arts and music. They include such diverse functions as the Dublin Horse Show, recitals of classical music and the provision of radon for therapeutic purposes throughout Ireland. The bicentenary celebrations will be held at the society's headquarters at Ball's Bridge, where ample accommodation is available for the large gatherings that a membership roll of nine thousand is likely to entail, during the period June 23–27. The functions will include an opening conversazione, special scientific and general meetings (the latter on the bicentenary date, Thursday, June 25), a garden party and a period ball. In addition to these functions at Ball's Bridge, their Excellencies the Governor-General of the Irish Free State and Mrs. McNeill have kindly promised to invite the special guests of the society to a garden party which will be held in the grounds of the Viceregal Lodge on Wednesday, June 24. An exhibition will be staged in some of the halls and grounds illustrating the advances made in agriculture, industry, science and art in Ireland during the past two centuries. An interesting feature of the bicentenary week will be the presentation to Sir John Purser Griffith of the Society's Boyle Medal, which has recently been conferred on him in recognition of his work in engineering science.

DISCUSSION

NEW OSTRACODERMS FROM OESEL

LAST summer Dartmouth College generously financed my third expedition to the Island of Oesel in the Baltic Sea, where I hoped to find new material for work begun some forty years ago on the "Origin of Vertebrates." This island is famous for the abundance and beautiful preservation of some of the oldest forms of animal life. Among its fossils of the upper Silurian age are many sea scorpions, or eurypterids, which for untold ages had been the highest animals in existence. Mingled with them are several kinds of ostracoderms, a great class of primitive and highly diversified fish-like animals, which at about this geologic period were making their first appearance on the historic screen. We have for many years regarded the ostracoderms as the remote Cambrian, or pre-Cambrian, descendants of the sea scorpions, and the ancestors of the long line of true fishes, reptiles and mammals which hundreds of million years later culminated in man. For in spite of

the obvious differences between them, the fundamental pattern of bodily structures and functions in all these different forms is essentially the same, and quite unlike that in any other known kind of animals. For that reason, and because of the suggestive sequence of their appearance in geologic time, together with abundant embryological evidence derived from the study of modern representatives, and especially because of the remarkable anatomical evidence provided by the oral arches of the new fossils from Oesel, the ostracoderms and the sea scorpions may now be regarded, beyond any reasonable doubt, as genetically related. In other words they really are the long sought missing links between the highest invertebrates of those very early times and all the vertebrates that arose in subsequent geologic periods. Many fundamental problems of comparative anatomy, embryology, and organic evolution are dependent for their solution on the recognition of the genetic relations of these two great types of animal life.

The paleontological key to the origin of vertebrates is the structure and arrangement of the chief sense organs of the ostracoderms (such as taste, sight, smell and hearing) and that of the several pairs of jaw-like arches on either side of the mouth. For these grasping and searching sentinels, posted around the main entrance to the body, play conspicuous rôles in the subsequent evolution of the head and face in all the higher vertebrates. And these external features, as always, most clearly express the character of the life within.

There is one little ostracoderm, of which there are many kinds in different parts of the world, in which these organs are enclosed in highly polished and exquisitely modelled bony plates. Although they are usually badly crushed, or scattered about in the muddy sediments now turned to rock, they may, and often do, provide us with exact information as to the nature of these organs, some five hundred million years ago. It was this particular kind of ostracoderm, called Tremataspis, that we were looking for. The animal is about three inches long. It is found only in the island of Oesel, and even there its remains are very rare and always fragmentary.

Our excavations for these fossils were the first ones, on a large scale, that had ever been made in Oesel. We spent some seven weeks in the field, using for most of that time a crew of from fifteen to twenty-seven native workmen, ten hours a day. Four or five feet of rock and soil were removed from an area of about four hundred square yards. The lower, fossiliferous layers, some two or three feet thick, were split into thin slabs, in order, if possible, to locate, or partly expose the more complete specimens without injuring them. The slabs were then broken up into small hand pieces and carefully searched for certain bony plates some of them not much larger than the head of a pin.

After exhausting the old site, near Kiehelkond, we moved to Atla, where there is a peasant quarry recently explored by Professor Luha, an Esthonian geologist from the University of Dorpat. Here we found six new species of ostracoderms. One represents a new family and belongs to a new genus that I have called Dartmuthia. One is a new cephalaspid, and another is a small fish, uniformly covered with loose scales and belonging to an order that looks very strange in these surroundings.

Four of the new forms are as follows:

Tremataspis milleri, n. sp. Branchiocephalic shield, 45 x 36 mm; highly polished; olfactory opening in bottom of deep pit; six to eight dorsal tubercles; occipital crest high, sharp-edged, and overhanging behind. Named after a friend and supporter of the 1928 expedition.

Tremataspis mammillata, n. sp. Shield 39 x 28 mm. Olfactory opening level with or above surrounding surface. Twenty or more small dorsal tubercles.

Didymaspis pustulata, n. sp. Shield about 19 x 29 mm; semi-membranous, flexible; outer surface minutely spiculate; inner surface divided into large, well-marked polygonal areas, with corresponding pustular elevations externally, each one capped with a glistening nodule. Two pairs of marginal areas; distinct oral plates. This form is new to Oesel and gives us for the first time a clear picture of a little known genus.

Dartmuthia gemmifera, n. f., n. g., n. sp. Branchiocephalic shield completely united; 53 x 39 mm; no cornua; one pair of marginal areas. Outer surface smooth and continuous, but divided into minute polygonal areas studded with large gem-like tubercles loosely distributed on dorsal surface, but closely packed on under side of margins, and merging into feather-like ornaments near the gill openings. Ventral post-branchial surface covered with flat, closely united polygonal plates. This represents a new family of ostracoderms, in some respects intermediate between the *Cephalaspidae* and *Trematаspidae.*

The new species of *Trematаspis* and the specimens collected two years ago now give us an almost complete picture of the external structure of these remarkable animals. Two important plates, which I formerly interpreted as parts of oar-like cephalic appendages, have been found in place, united with their associated anatomical parts. One is a convex anal plate, located at the root of the tail and well within the branchiocephalic shield, as in *Bothriolepis.* The other is a fin-like dorsal trunk spine. Nevertheless we have found indications of at least one pair of cephalic appendages on the margins of the circumoral

region. We have also found well-defined membranous flaps protruding from the posterior opening of the branchiocephalic shield in *T. mickwitzi.*

But the most significant discovery was the finding of one and probably two pairs of jaw-like crushing plates in their natural position in two widely different species, *T. mickwitzi* and *T. schmidti.* In both species, they are definitely located on either side of a slit-like longitudinal mouth which is ventral in position, not terminal; and the jaws evidently work sidewise against one another, not forwards and backwards, as they do in typical vertebrates.

Moreover in *Dartmuthia* and *Tremataspis* there are four pairs of conspicuous endoskeletal plates and processes arising from the inner lateral surface of the cephalic shield and pointed towards the mouth. They evidently serve, in part, for the attachment of four sets of muscles and clearly indicate that there are really four non-respiratory oral segments in front of and serially homologous with the eight respiratory gill segments. All this agrees with the location of the several pairs of oral arches (premaxillae, maxillae and mandibles) in the embryos of the higher vertebrates. It also agrees with the postulates and predictions of the arachnid theory of the origin of vertebrates.

WILLIAM PATTEN

DARTMOUTH COLLEGE

THE OCCURRENCE OF OLD MEADOW SOD UNDER THE NEW JERSEY BEACHES

A STUDY of the changes in the position of the shoreline of any coast is very important, but along the New Jersey coast such a study is of particular significance because of the immense amount of money invested in the summer resorts of that state.

There has been some difference of opinion in regard to the question of whether the coast of New Jersey is actually sinking at the present time. Some seventy-five years ago, Dr. George Cook, then New Jersey state geologist, presented evidence which he thought showed that the coast of Cape May County was sinking at the rate of two feet a century or one quarter of an inch a year. Others have expressed the same opinion.

More recently, however, it has been shown that the changes in shore-line may have been brought about by factors other than the subsidence of the land, mainly the erosive action of the waves and currents on the sand beaches. Dr. Douglas Johnson, who has studied the situation thoroughly, says that the evidence favors unusual stability of the land during the past few thousand years.

No matter which interpretation we accept, there still remains undeniable evidence of marked changes in the position of the shore-line along this coast. Cook in the report of the New Jersey state geologist for 1881 pointed out that at numerous places along the coast the wearing away of the beaches had exposed old salt meadow sod on the ocean shore. Since there is no such sod along the shore outside the beaches, this old sod must have grown there when it was a part of the meadow between the beach and the upland, thus indicating a considerable change in the position of the shore-line. At certain places in this sod were to be seen the stumps of old trees, suggesting that the region at one time supported an upland association.

In his volume for 1882 Dr. Cook says that there has been a common report that these meadow sods along the sea border, in some places which were uncovered by violent storms, were plainly marked with the tracks of horses, cattle and sheep. After the severe storm of September 21, 22 and 23, 1882, such tracks were plainly visible a few miles south of Harvey Cedars, Long Beach Island, N. J. They were found in a patch of old meadow sod about three feet below ordinary high-water mark. The sod was thickly marked with the tracks of horses and cattle. The horse tracks were of various but rather small hoofs and without shoes, and the cattle tracks were also of various sizes. The sod and tracks extended back under the hillocks of beach sand.

At that time (1882) that part of the beach had few if any domestic animals on it, but in 1690, when it was settled, horses and cattle were kept on the island which at that time extended considerably farther out to sea.

In the sod near these tracks were seen the stumps of numerous trees and bushes.

Some fifteen years ago, similar tracks of cattle, horses and birds were reported in sod near South Cape May, N. J., exposed after severe storms.

In the last few years patches of this old meadow sod containing the stumps of trees, roots of grass, etc., have been seen in several places along the beach in the vicinity of Cape May. After the heavy seas of early January, 1931, some three feet of sand was eroded from the beach at Cape May Point, exposing the old sod at several places. Stumps of red cedar trees and roots of various plants were seen. Near low-water mark, close to the Cape May Point Coast Guard Station, was seen very clearly the remains of an old corduroy road leading from the present shoreline out toward the sea in the direction of Prissy Wick Shoal, about one mile distant. Tradition says that less than one hundred years ago this shoal was above water and was separated from the present shoreline by low-lying land, and that it was possibly the site of the original Cape May Lighthouse. A study

of maps of the region in the time of the Revolutionary War shows that the land extended considerably farther out than it does at present, and that there was a road approximately in the position of the corduroy recently uncovered; it therefore seems quite probable that this corduroy road is the old road leading to the now submerged Prissy Wick Shoal.

On April 6, 1931, the road was again exposed, this time more distinctly. On a patch of sod, about one tenth of a mile west of the log road, were seen several horse footprints. As far as can be learned, this patch of sod had not been uncovered for at least several years. This locality is about one half a mile distant from the one near South Cape May where the tracks had been seen some fifteen years previously.

The fact that these tracks were of shod horses, while those reported by Cook at Harvey Cedars were of unshod animals, suggests that these tracks may not be quite as old as those previously seen. Nevertheless, the fact that these tracks have persisted in this sod superimposed by a thin layer of sand and covered by the sea twice a day seems interesting and suggestive of how fossil tracks are actually preserved. These horse tracks may possibly be regarded as "fossils in the making."

A more detailed and illustrated account of the occurrence of this meadow sod beneath the New Jersey beaches will be published elsewhere in the near future. This preliminary note is published in the hope that some one may report similar occurrences elsewhere.

HORACE G. RICHARDS
UNIVERSITY OF PENNSYLVANIA

TWISTED TRUNKS OF APPLE TREES

THE recent discussion relative to the twist in the trunks of certain trees has been interesting. I have carefully examined apple trees in orchards from Iowa and Minnesota to New York and Pennsylvania, and have noted that a large percentage of old trees are strikingly twisted. The twist has nearly always been to the right. The variety of apple does not seem to make any difference. It is more likely a matter of age. At least it is more readily seen in the older trees. Soil and position of the orchard does not change the character of the twist. In some orchards nearly 100 per cent. of the trees were twisted. It probably has nothing to do with wind or weather, but is more likely a form of tropism. A great many climbing plants twist in the same direction. Many species of trees also twist the same way. So far as I have observed, a twist to the left is rare. I have been told that in the southern hemisphere the twist is dominantly to the left. If this is so, then the condition is surely the result of the influence of sunlight and position with respect to the equator. In this respect it is like the trade-winds. I am strongly inclined to believe that the twist is the direct result of the influence of sunlight, similar to the turn of the sunflower and the leaves of the compass-plant of the western prairies.

BERTRAM T. BUTLER
COLLEGE OF THE CITY OF NEW YORK

EARLY UTILITARIAN APPLICATION OF TWIST IN TREES

THE twist in the grain of coniferous and deciduous trees discussed by Chas. K. Wentworth in SCIENCE, February 13, and by Arthur Tabor Jones in the issue for March 27 was advantageously adapted to the service of agriculture in America in the 18th and the early part of the 19th centuries. Trees having a left-hand twist were then used in the construction of the mold-board portion of the so-called "wooden plow" of that period.

The length of the mold-board was determined in a measure by the angle of the grain twist since its strength depended upon the extent to which cutting across the grain of the wood became necessary in the shaping of its warped surface. The length of the wooden mold-board was, for this reason, considerably greater than that of the present day all metal plow. Clearly enough a large size hardwood tree having a close left-hand twist was greatly prized by the plow maker as he was able to secure from such a tree the raw material for the mold-boards of several plows.

When the mold-board, land-side, handles and other portions of a plow had been assembled all parts that came into contact with the soil in plowing were armored, or as it was then termed "plated," with thin wrought iron straps and plates formed to fit the wooden parts to which they were riveted. At that time all bolts and their nuts were hand made and were, therefore, more costly than hand-made rivets and key-bolts.

The occupation of "wooden plow" making was entirely confined to the individually owned small-shop period of American manufacturing industry. The plow maker, assisted, possibly, by one or more apprentices—men legally bound by agreement (articled) to his service for a period of years—performed every portion of the work. He selected the twist grain trees in the woods, cut and hauled them to his shop, attended to the proper seasoning of the wood and in the actual manufacturing operations became carpenter and blacksmith in turn. He marketed his finished product and for the most part received therefor other goods in exchange rather than real money.

One of these early American "captains of industry" was a Nathaniel Edwards, who was born June 21, 1752, Haverhill, Mass., and who died June 14,

1828, Casco, Maine. He was commonly known as "Plowmaker Nat."

LLEWELLYN N. EDWARDS
BUREAU OF PUBLIC ROADS,
U. S. DEPARTMENT OF AGRICULTURE

PANAMAN

THE latest editions of the Standard Dictionary and of Webster's International Dictionary give the preference to Panaman as the adjectival form of the word. Both dictionaries give Panamaian (Pan-a-ma-yan) and Panamanian as alternative forms. Both the noun Panama and the adjective Panaman carry the accent on both the first and last syllables, the antepenult and the ultimate, and not on the ultimate alone.

The accented ultimate or final syllable is very common in Spanish proper names and other words, as it is also in Persian place names. When the adjectival form of such a proper name is created in English, however, this adjective becomes subject to the rules of the English language and the accentuation of the original language need no longer be followed. There is much evidence that, in America at least, the accented ultimate is giving way to the accented antepenult, thus, Pan′ a man, with the secondary accent on the ultimate. Incidentally, this seems to be the prevailing pronunciation of the noun Panama among even the well-educated Americans.

CARLETON R. BALL
UNIVERSITY OF CALIFORNIA

CURE FOR FORMALIN POISONING

IN SCIENCE for May 8, 1931, appeared a discussion of formalin poisoning with an appeal for a remedy. About four years ago I developed a most irritating case of this poisoning on my fingers. I tried various remedies and doctors for two years with no success. Then Dr. W. E. Tebbe recommended that I use lanolin. He explained that the formalin kills the sweat glands and that the only way to restore them is to use an animal fat which can be absorbed. The result has been most satisfactory. All trace of the poisoning disappeared in six months. I find that I can handle preserved specimens with safety now if I apply the lanolin at the first indication of irritation.

VESTA HOLT

BARRO COLORADO ISLAND BIOLOGICAL STATION
(1930–31)

THE seventh annual report of this tropical biological station, as presented by Dr. Thomas Barbour, chairman of the executive committee of the Institute for Research in Tropical America, includes the following items covering the year ending February 28, 1931.

Several additions to the plant are reported, particularly a building at the end of the Pearson Trail. This structure is made entirely of lumber treated by the zinc-meta-arsenite process as a termite resistance experiment in cooperation with the Curtin-Howe Corporation, which controls the process, and the Bureau of Entomology of the U. S. Department of Agriculture. It is fully equipped for use as a residence by any visiting naturalist and located in the vicinity of innumerable bayous and with great diversity of habitats near at hand. The mangosteens and other planted trees are growing finely, the trails have been well cleared, bridges put in good condition and in general the plant is in excellent order.

Mr. Zetek, the indefatigable resident custodian, has prepared a card index of all publications referring to the island, arranged by author and subject, and is continuing the species index begun last year. It is requested that all investigators inform him at the earliest possible time of identifications that are made. Since the species index was started, Dr. Herbert N. McCoy has twice given financial assistance. Several other donations consisting of apparatus are mentioned.

A condensed statement of the facilities which the laboratory offers and the concessions granted workers by the government of the Panama Canal and by steamship companies, etc., has been printed and may be obtained from the office of the chairman (Dr. Thomas Barbour, Museum of Comparative Zoology, Cambridge, Massachusetts) or resident custodian. There have been no changes in the steamship arrangements announced in the last annual report, when they were discussed in full. One misstatement, however, was made at that time; the special rate offered by the United Fruit Company is $75 per round trip, and not each way.

A list of seventeen investigators in residence at the laboratory for extended periods during the year is included in the report, together with brief statements of their studies. The published papers resulting from studies at the laboratory now total 148 as compared with the 118 titles last year. The current additions are listed with comments in special cases, and there are lists of the mammals, molluscs, termites, fruit flies and trypetidae. The amphibia and reptilia are listed as known from the Canal Zone as a whole.

Under "Present Needs" it is stated that "the island is badly in need of a simple electric installation to furnish light and power. The dynamo should be located on the dock where fueling would be convenient and this innovation would not only be a great convenience and an aid for work in the evening, but would enormously lessen our fire hazard. The total cost would not exceed $750 for a one and one half kilowatt unit."

The greatest need of the laboratory is an adequate endowment. At the present time nine institutions are subscribing for tables at $300. Donations total $600, and there are various minor sources of income which make the total receipts $5,583.10. An endowment that would be modest compared with that of many biological laboratories would greatly increase the effectiveness of the station. Dr. Barbour believes "there is no place in the world where so small a sum would so greatly aid biological research." The following resolutions adopted by the Inter-American Conference on Agriculture, September 13, 1930, illustrate the esteem in which the studies being conducted and those possible at Barro Colorado Island are held by tropical agriculturalists:

The Inter-American Conference on Agriculture, considering that

Whereas, the Department of Agriculture of Porto Rico, the experiment station of the United Fruit Company in Tela, Honduras, and the biological station in *Barro Colorado,* in the Canal Zone, have been conducting investigations along special lines of tropical agriculture and forestry, and making the results of this work available as far as possible to several Latin American countries;

Resolved, (1) To express appreciation for these valuable services, and the hope that they will be further expanded, and that in the future closer cooperation will be established with other experiment stations and agencies of scientific research in the countries of America.

(2) That an endeavor be made to obtain the cooperation of the experiment stations in the countries of America now equipped to render a Pan American service, such as the experiment stations of Porto Rico, the experiment station of the United Fruit Company in Honduras, the Barro Colorado Island Biological Station in Panama, and stations in other countries of America which have facilities for such services for special investigations of problems the solution of which is most urgent for agriculture, forestry and animal husbandry in the countries of tropical America.

There is the further need of support for studies without immediate utilitarian possibilities. To this end the Institute for Research in Tropical America, which is the organization legally back of the Barro Colorado Laboratory, is seeking an endowment of $100,000. This proposal received endorsement by the executive board of the National Research Council at its meeting in April, 1931. The laboratory has demonstrated its usefulness and should be relieved of its present financial uncertainties.

W. C. CURTIS,
Chairman Division of Biology and Agriculture, National Research Council

QUOTATIONS

THE CAPPER AWARD

WHEN Ossian heard "the call of years" he lamented that no bard would "raise his fame." But the great entomologist Dr. L. O. Howard, whose middle name recalls the legendary Gaelic hero of the third century, needs no poet to sing his deeds in fighting for a half century the forces which "constitute to-day our greatest rivals in the control of nature"—the injurious insects. He has been recognized in a more substantial and significant way: he has been awarded the Capper Gold Medal for distinguished service to agriculture, and through it to those who live by it or on its fruits. The award also includes an honorarium of $5,000.

No one in all the world better deserves such recognition than this entomological warrior in "the oldest war in history," between mankind and the insect myriads. The only hope that the human race has of winning is in uniting its scientific forces in research and attack and in dividing the enemy—encouraging conflicts among the insects themselves, even nourishing parasitic battalions in laboratories to prey upon other insects and so maintain a balance that will permit crops to grow, flowers to bud and blossom, trees to bear fruit and the "higher" creatures to live and pursue happiness.

Dr. Howard has been and is a master of such strategy in fighting these lilliputian enemies, which are much more experienced in the ways of this planet, having lived here, as he reminds us, 50,000,000 years, while man arrived barely 500,000 years ago, and are "the most perfectly adapted of all creatures to live under all sorts of conditions." Fortunately for man, they fight among themselves and prey upon one another—the fleas on smaller fleas, and so on, as Jonathan Swift said in reporting the naturalist's observations, *ad infinitum.* But the surpassing achievement of this master entomologist has been to recruit insect allies and mercenaries from the lower biological orders for his campaigns against specific pests, even bringing them from other lands and sending American expeditions overseas to aid other countries.

Yet the warfare is not over. A few years ago Dr. Howard estimated that the annual loss due to the ravages of insects in the United States alone exceeded $2,000,000,000, nullifying the labor of 1,000,000 men annually. And as to the recruiting by the

enemy, he quotes approvingly even later and astounding statistics which assert that the plant lice descended from one individual of one species in a single season, where there is enough food, would weigh more than five times as much as all the people of the earth.

The award to Dr. Howard calls attention not only to his valiant service as a leader in this warfare, but also to the importance of the struggle in the agricultural world, where only the ingenuity of man can prevent the supremacy of the insect.—*The New York Times.*

SOCIETIES AND ACADEMIES

THE ILLINOIS ACADEMY OF SCIENCES

THE twenty-fourth annual meeting of the Illinois State Academy of Science was held in Peoria on May 8 and 9, 1931. General addresses were given on the following subjects:

"Research, Its Opportunities and Rewards," F. R. Jelliff, Galesburg, retiring president.

"Genesis of an Industry," W. Hoskins, Chicago.

"Physics and Physical Chemistry," T. R. Hogness, University of Chicago.

"Chemical Messengers," A. C. Ivy, Northwestern University Medical School.

"From Chance to Certainty in Education," F. G. Blair, Superintendent of Public Instruction, Springfield.

"Saving Illinois Streams from Pollution," H. F. Ferguson, Department of Public Health, Springfield.

The following resolution was adopted:

Realizing the large value and great importance of research along many lines and the benefits accruing to the people from inventions, explorations and discoveries in science, often the result of patient, persistent and painstaking endeavor, resolved that the Illinois State Academy of Science, while fully appreciating the recognition accorded such work, would respectfully recommend that Congress add to this the establishment of financial awards for the most noteworthy and valuable inventions and discoveries in the several branches of science, to be bestowed under such conditions as Congress may direct.

The officers elected for the year 1931–32 were:

President: Fay-Cooper Cole, University of Chicago.

First Vice-president: Frank C. Baker, University of Illinois.

Secretary: Harold R. Wanless, University of Illinois.

Treasurer: George D. Fuller, University of Chicago.

Librarian: A. S. Coggeshall, State Museum, Springfield.

Editor: Dorothy E. Rose, State Geological Survey, Urbana.

The following were chosen as chairmen of committees:

Membership: D. L. Carroll, State Geological Survey.

Affiliation: H. J. Van Cleave, University of Illinois.

Ecological Survey: A. G. Vestal, University of Illinois.

Conservation: H. C. Cowles, University of Chicago.

Legislation and Finance: F. R. Jelliff, Galesburg.

State Hall of Fame: M. M. Leighton, State Geological Survey.

Sectional chairmen selected for the next annual meeting are:

Zoology: F. C. Hottes, Millikin University, Decatur.

Physics and Chemistry: C. L. Cross, Illinois State Teachers' College, Normal.

Geology: T. E. Savage, University of Illinois.

Geography: Mabel Crompton, Illinois State Teachers College, Normal.

Psychology and Education: M. M. Maynard, Monmouth College, Monmouth.

The meeting was attended by about 800, including a large delegation of the junior section of the academy. Science exhibits prepared by high-school students were shown. Geological, biological and industrial field trips were taken to points of interest near Peoria on May 9.

H. R. WANLESS,
Secretary

THE TENNESSEE ACADEMY OF SCIENCE

THE spring meeting of the Tennessee Academy of Science was held at the University of Tennessee, in Knoxville, on Friday and Saturday, May 8 and 9. East Tennessee was represented on the program with sixteen papers and Middle Tennessee with nine. After a dinner on Friday evening the members by invitation of the University Student Body attended a lecture by Mr. Lorado Taft, sculptor, on "My Dream Museum." At the dinner Professor H. A. Webb substituted with a humorous pseudo-scientific narrative for Dr. E. E. Reinke, who on account of illness was prevented from giving an address on "A Mountain Station in the South for Biological Research." A trip to the Bird Preserve, near Knoxville, scheduled for from 6 to 8 o'clock Saturday morning and an excursion to the Great Smoky Mountains for Saturday afternoon had to be given up on account of a downpour of rain.

Mr. Henry Colton and Dr. L. C. Glenn were appointed a committee on State Aid to the Academy. The editor of the *Journal* was authorized to proceed

on a policy of increasing the exchange list and as he sees best respecting advertising in the *Journal*. Mr. Latimer J. Wilson was elected a delegate to the meeting of the American Association for the Advancement of Science at Pasadena in June, and Dr. J. T. McGill to the meeting at New Orleans in December.

The officers of the Academy for 1931 are:

President: L. R. Hesler, University of Tennessee, Knoxville.

Vice-president: H. A. Webb, George Peabody College, Nashville.

Editor: Jesse M. Shaver, George Peabody College, Nashville.

Secretary-Treasurer: John T. McGill, Vanderbilt University, Nashville.

SCIENTIFIC APPARATUS AND LABORATORY METHODS

A TRANSPARENT ELASTIC GLUE, USED IN MAKING CHAMBERS FOR INSERTION IN THE RABBIT'S EAR

IN connection with the new methods for studying the growth and reactions of living cells and tissues in the living mammal, originated and developed under the direction of Dr. E. R. Clark, it became necessary to find a satisfactory glue or cement substance which would fasten together various parts of the transparent chambers used for insertion into the rabbit's ear.

The first type of these chambers was developed by Dr. J. C. Sandison ('28)[1] and was made entirely of kodaloid, and the various parts were stuck together by parlodion. Later a number of workers in this laboratory collaborated on various improvements in the technique in order to obtain standardized chambers which would give uniform results and would be adapted to various types of observation and of experiment (Clark, Kirby-Smith, Rex and Williams, '30).[2] The thin kodaloid top proved to be unsatisfactory for such standardized chambers because of its tendency to warp and to allow the escape of moisture. Glass covers were much too fragile. Mica proved to be a satisfactory substitute as regards thinness and clearness, and the finding of a satisfactory glue to seal mica to heavy kodaloid or glass, used in the bases and supporting rings of the chamber, has obviated the chief difficulties inherent in the use of mica in the earlier chambers (Sandison, '24).[3]

A satisfactory glue for use in the construction of the chambers had to meet a number of requirements. It was necessary for it to be permanently adhesive and to be impervious to and unaffected by fluids, including the natural tissue fluids and antiseptic solutions such as phenol, hexyl-resorcinol and metaphen, and to be uninfluenced by moderate changes in temperature. In addition, it was highly desirable for it to be elastic, transparent and smoothly clear (without bubbles).

A large number of experiments were carried out before a glue which meets all these requirements was obtained. Balsams and resins of different varieties were tried with many different solvents. A number of varnishes and shellacs were also experimented with. Different commercial cements were tried. Celluloid compounds in different mixtures and combinations were used. Some of these substances, such as glyptal, passed the tests with water, but failed after the chamber was placed in one of the disinfecting solutions, or after insertion in the ear. Others (especially the cements such as Duco) were successful in sticking mica to glass, but had a tendency to warp the heavy kodaloid and to form bubbles.

The present glue forms a permanent, tenacious cement. It is smooth, transparent and waterproof, is unaffected by the moisture of the animal's tissues, by various antiseptics, or by moderate changes in temperature, and possesses the added advantage of elasticity. It will stick mica to kodaloid, to glass or to silver, kodaloid to glass or silver, and glass to glass.

The ingredients used and method of preparing the glue are as follows:

Pure gum copal (in lumps, *not* powdered)
Venice turpentine
Xylol

Select lumps of the copal which are clear and light amber in color. Heat copal in a porcelain dish until melted. While still over the flame, add a small amount of Venice turpentine and stir well. (The amount of Venice turpentine depends on the desired flexibility of the cement). Turn off the flame and continue stirring while adding xylol in small amounts. Some of the xylol evaporates, and it is therefore advisable to add a little xylol continuously while the

[1] J. C. Sandison, "The Transparent Chamber of the Rabbit's Ear, Giving a Complete Description of Improved Technic of Construction and Introduction, and General Account of Growth and Behavior of Living Cells and Tissues as Seen with the Microscope," *Am. J. Anat.*, Vol. 41, No. 3, p. 447, 1928.

[2] E. R. Clark, H. T. Kirby-Smith, R. O. Rex and R. G. Williams, "Recent Modifications in the Method of Studying Living Cells and Tissues in Transparent Chambers Inserted in the Rabbit's Ear," *Anat. Rec.*, Vol. 47, No. 2, p. 187, 1930.

[3] J. C. Sandison, "A New Method for the Microscopic Study of Living Growing Tissues by the Introduction of a Transparent Chamber in the Rabbit's Ear," *Anat. Rec.*, Vol. 28, No. 4, p. 281, 1924.

cement is still hot. On cooling, the cement may become hard. This indicates an insufficiency of xylol, and it is then necessary to reheat the mixture and add more xylol. When cool the glue should have the consistency of molasses.

In applying the glue a small camel's hair brush is used. The consistency of the glue allows plenty of time to apply it smoothly and in the exact amount required.

Copal glue thus prepared is not sufficiently tenacious to hold pieces together when strong forces are exerted which tend to separate the pieces. Consequently it can not be used for the original type of chamber described by Sandison, which was glued together before insertion into the ear. But it is tenacious enough to hold together either the tops or the bottoms of the newer types of chamber, since the forces exerted are such as to press the glued portions closer together, while the forces which act so as to separate the top from the bottom are resisted by nuts and bolts.

When parts of the transparent chambers have been cemented in the manner described recently (Clark *et al.*, '30) it is necessary for them to stand for at least 24 hours—preferably longer—before insertion in the rabbit's ear, on account of the susceptibility to irritation on the part of living tissues toward a trace of free xylol.

Although up to the present time this glue has been used only for the purpose for which it was invented, its qualities should prove useful in sealing total mounts, especially of specimens cleared in oil of wintergreen.

Thanks are due to Dr. S. E. Pond for information regarding types of glues, to Dr. O. V. Batson, who suggested the use of copal, and to Dr. E. R. Clark, at whose instigation the studies were made.

B. B. VARIAN

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CELLOPHANE COVERS FOR PETRI DISHES FOR KEEPING OUT CONTAMINATIONS AND STUDYING THE EFFECTS OF ULTRA-VIOLET LIGHT

ORDINARY petri dishes, with loosely fitting glass covers, are not altogether satisfactory for use in research on pure cultures of certain organisms, particularly fungi, because of the difficulty of preventing contaminations. By means of a simple technique covers of cellophane may be applied, making it possible to keep the cultures almost indefinitely without danger of contamination. Furthermore, they may be examined as often as desired under the low power of a microscope, without exposing them to contamination, for the flexibility of the cellophane makes it possible to bring the objective close to the organism.

In addition to maintaining the purity of cultures, cellophane offers a tremendous advantage in the investigation of the effects of ultra-violet light on various organisms in pure culture. Ordinary glass covers transmit hardly any of the ultra-violet spectrum, and the best of the special ultra-violet transmitting glasses are impenetrable to the very short wavelengths. Cellophane, on the other hand, is nearly as transparent to the extreme ultra-violet as air, and as it is only .025 to .03 mm thick, the percentage transmission, as compared to air, must be close to 100. Cultures may thus be irradiated by any wave-length of ultra-violet, from the shortest to the longest, over any length of time and for any duration of exposure, by placing an appropriate filter on the cellophane cover, without exposing the culture to contamination. In some experiments, now almost complete, conducted on several species of fungi, some very interesting results were obtained by means of this procedure. It is a distinct improvement over former methods, in which the glass cover of the petri dish was removed in order to study the effect of the extreme ultra-violets.

The application of the cellophane covers is quite simple and not time consuming. The percentage of contaminated cultures, after a period of a month, was reduced from about 20 per cent. to 0.5 per cent. by applying cellophane covers according to the following method. Some of the cultures were carried around in the pockets of my coat for several days without subsequent contamination.

The cellophane is cut into square sheets, about 6 by 6 inches, and sterilized by placing in 60 per cent. alcohol in a flat glass dish for half an hour. The cultures are inoculated in the usual manner. It is necessary to exercise some care in transferring the cellophane onto the petri dish. Best results are obtained by placing the petri dish culture next to the dish containing the cellophane saturated with alcohol, lifting the top of the petri dish with the thumb and forefingers and then drawing a sheet of cellophane across the top of the bottom part of the petri dish with the third and fourth fingers, thus avoiding at any time exposing the culture to any bacteria or spores that might otherwise fall into it. The glass cover can now be replaced to press down the cellophane, and then removed again and a rubber band applied to hold the cellophane around the sides. If this is done carefully, no alcohol will get into the medium, and in a few minutes it will evaporate out of the cellophane, leaving it perfectly dry, transparent and tightly stretched across the top of the dish. If the culture is to be kept for any length of time,

it is necessary to replace the glass cover of the petri dish to keep the medium from drying up. It may be removed, however, as often as desired, in order to examine the culture with a microscope, or irradiate it with ultra-violet light. Cultures can be conveniently labeled with india ink directly on the cellophane.

FRANK H. JOHNSON

PRINCETON UNIVERSITY

SPECIAL ARTICLES

THE EFFECTS OF BREED ON GROWTH OF THE EMBRYO IN FOWLS AND RABBITS

T. C. BYERLY[1] has recently made an important study of the weight of chick embryos in two different breeds of domestic fowls and in their reciprocal hybrids. He reaches the conclusion that breed does not affect the size of the embryo except as it affects the size of the egg previous to incubation. This is contrary to the conclusion reached by Painter[2] and by Castle and Gregory[3] in the case of the rabbit, which leads Byerly to question the correctness of the rabbit findings.

In the rabbit studies it had been found that the size of the egg at the time of fertilization is no greater in Flemish Giant rabbits than it is in Polish (a very small breed), but that the average birth weight of a Flemish Giant is nearly double that of a Polish. It is obvious accordingly that Flemish embryos increase in weight faster than Polish embryos prior to birth, as they are well known to do subsequently. Since there is no discoverable difference in cell size between Flemish and Polish rabbit embryos (Painter), it is clear that the former must contain *more* cells, and this means that cell multiplication must proceed more rapidly in the development of Flemish than in that of Polish rabbits. Castle and Gregory have found such a difference in evidence as early as 48 hours after mating. Byerly questions the adequacy of the data submitted in support of this conclusion. To this criticism we offer no objection at this time because we have made additional observations, which will be presented in a paper[4] now in press, showing that the difference in number of blastomeres and in mitoses is clearly present at still earlier stages, *viz.*, 40 and 41 hours after mating.

The case of the chick embryo is more difficult because the size attained by the embryo at the time of hatching, which corresponds roughly with the birth weight of the rabbit, is strictly limited by the weight of the egg prior to incubation. A large chick can not hatch from a small egg. Nevertheless, it is possible to derive from Byerly's observations clear indications as to whether breed (*i.e.*, genetic constitution), does or does not influence embryo size prior to hatching, while there is still an unexhausted supply of nourishment for the embryo to draw upon.

The two breeds studied by Byerly in pure matings and in reciprocal cross matings were White Leghorn and Rhode Island Red, which for brevity we may call the White and the Red breeds, respectively. Red hens average about one third larger than White, or as 100 : 138 in mean body weight. The mean egg weight of the Red breed was also slightly greater, 60.5 grams as compared with 58.4 grams, the mean egg weight of the Whites. Whether the energy content of the Red egg is greater is unknown, as the relative weight of shell and relative size and composition of the yolk are unknown. Byerly directs his attention chiefly to a comparison of the weight of the embryo when removed from the yolk in White as compared with Red eggs throughout the incubation period. It appears from his observations that the blastoderm of the egg, when removed from the yolk, prior to incubation is heavier in the White breed than in the Red. For the White breed, the mean weight is 0.0030 grams; for Reds, 0.0028 grams. Whether the difference is due to a larger amount of formed cellular material[5] or to a larger amount of adhering yolk is unknown, but whatever its nature, the difference persists throughout the first nine days of incubation, in which the embryos taken from White eggs are slightly heavier than those taken from Red eggs. Subsequently, *i.e.*, from the 10th to the 19th days of incubation, the Red embryos are heavier. This is shown both in Byerly's Table 1 summarizing his more numerous observations and in his Table 3 summarizing the data obtained under specially controlled conditions, "from hens of the same age and receiving the same diet, from eggs of the same weight and incubated in the same incubator at the same time."

Nevertheless the hatching weight of chicks in the two breeds is substantially the same, which points to total egg size as a factor limiting the size of the chick prior to the time that it begins to receive nourishment from other sources.

The more rapid growth of Red embryos, after the initial handicap of a smaller blastoderm had been overcome, and before total egg size had entered as a limiting factor just prior to hatching, is completely

1 *Jour. Morphol. and Physiol.*, 50, December, 1930.
2 *Jour. Exp. Zool.*, 50, 1928.
3 *Jour. Morphol. and Physiol.*, 48, September, 1929.
4 *Jour. Exp. Zool.*, 59, April, 1931.

5 Possibly in the White breed cell increase in the blastoderm proceeds farther than in the Red breed before coming to a standstill previous to incubation. If so, we can understand why this initial advantage persists for several days before the more rapid growth rate of the Red breed overtakes it.

in harmony with the observations made on rabbits, in that it shows that the embryo of the larger breed grows faster when other conditions are equal.

Another and even clearer indication that breed (genetic constitution) affects the rate of growth of the embryo throughout the entire period of incubation (even before endocrine organs are established) seems to have been overlooked by Byerly. This is the more rapid growth of crossed as compared with uncrossed embryos. The difference in blastoderm composition in the two breeds prior to incubation, which obviously influences embryo weight up to the ninth day of incubation, may be completely eliminated by confining the comparison to the eggs of one breed at a time, comparing the size of embryos produced in White eggs fertilized by White males with that of embryos produced in White eggs fertilized by Red males, and also comparing the size of embryos produced in Red eggs fertilized by Red males with that of embryos produced in Red eggs fertilized by White males. In both cases Byerly's observations show the cross-bred embryos to be preponderantly heavier, whether the mother was White or Red.

The White eggs opened each day (Table I) range in number from 10 to 75 in each series (pure-bred and cross-bred). The pure-breds average heavier on 4 of the 19 days of incubation, *viz.*, the 2nd, 8th, 16th and 18th. On the 15 remaining days, including both the first and the last, the cross-bred embryos are heavier.

The observations made on Red eggs are less numerous but point to the same conclusion. The cross-bred embryo is in general heavier. The number of cross-bred embryos studied is smaller and does not cover every day of the incubation period, ranging from 4 to 11 embryos per day, but its indications are clear. The period covered is from the 2nd to the 19th days of incubation, omitting the 6th and 7th, and the 13th, 14th and 15th. Cross-bred embryos are heavier on all except two (the 9th and 18th) of the 13 days sampled.

As to the hatching weight, that of the cross-breds is slightly greater in the Red series and slightly less in the White series. Here available nourishment within the egg comes in as a limiting factor. If this were removed, by taking body weights a few weeks subsequent to hatching, cross-breds would undoubtedly be found again heavier, as is well known from other observations.

The specially controlled series of embryos produced by Byerly, from eggs of the same size incubated simultaneously side by side, summarized in his Table 3, confirms the conclusions based on his more general series summarized in Table I. The number of embryos studied is smaller, ranging from 2 to 14 per day in each of the four series, but the conditions under which they were produced make their evidence particularly important. Embryos from the eggs of White hens mated with White males are heavier on 3 of the 12 days sampled; *viz.*, the 2nd, 3d and 12th; cross-bred embryos from the eggs of White hens mated with Red males are heavier on the other nine days (4, 5, 8, 9, 10, 11, 16, 17 and 19). Embryos from the eggs of Red hens mated with Red males are heavier on 4 of the 12 days sampled; *viz.*, 9, 11, 16 and 19; cross-bred embryos from the eggs of Red hens mated with White males are heavier on the other eight days (2, 3, 4, 5, 8, 10, 12 and 17). With the small number of embryos examined, it is evident that random sampling affects the results here more than in Table I, which included larger numbers; nevertheless the general trend of the observations is clear and consistent with the results of Table I. *Other things being equal, a cross-bred embryo grows faster than one not cross-bred.*

There is, we think, no escaping the conclusion based on Byerly's own observations that breed (genetic constitution) does influence growth rate and through it body size. Embryos of the larger breed grow faster as soon as they have attained an even start. Also eggs of the same breed laid by the same flock of hens under identical conditions, if fertilized by males of their own breed, produce smaller embryos than are produced if fertilization is accomplished by males of the other breed. Are the cross-bred embryos heavier because they contain more cells or larger cells? We may take our choice of these alternatives. If they contain more cells, then cell multiplication must occur more rapidly in the larger embryo, exactly as it does in rabbits. If one chooses to assume that the cells are larger rather than more numerous in cross-bred embryos, the burden of proof rests with him, for Painter has not found it so in rabbits, but in any case it is obvious that a cross-bred embryo grows faster than one not cross-bred in birds as well as in rabbits.

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THE EFFECT OF DIET ON HOOKWORM INFESTATION IN DOGS[1]

THE investigations summarized in this brief preliminary report give an experimental demonstration

[1] From the department of helminthology of the School of Hygiene and Public Health of the Johns Hopkins University. This work was made possible by the aid of the International Health Division of the Rockefeller Foundation.

of a definite correlation in dogs between undernourishment and susceptibility to infection with the common dog hookworm, *Ancylostoma caninum.* They also show that hookworm infestations which develop in dogs on a deficient diet can be practically eliminated by placing them on an adequate diet.

The routine dog diet used in our laboratory consists of pig lungs, milk, bread and water. Small amounts of cod liver oil are added to this diet for puppies and for the experimental dogs when changed from a deficient diet. The deficient diet used, which was suggested by Dr. H. D. Kruse of the department of chemical hygiene of this institution, consists of 35 per cent. of corn starch by weight, 35 per cent. of dried ground peas, 29 per cent. of Mazola oil, 1 per cent. of NaCl (C. P.) and an abundant supply of water. This food was always given in sufficient quantities to satisfy hunger, the significant fact being that it is very deficient in vitamines and important minerals.

The course of the infestations in the dogs was carefully followed by fecal examinations by the Lane method and the Stoll dilution egg counting technique, and the total fecal output of each animal was routinely screened for the recovery of any worms passed.

In the first experiment, five dogs were used. Two of them had been born in the laboratory and the other three had been in the laboratory since they were only a few weeks old. These five dogs had been used by Dr. O. R. McCoy[2] for studies of resistance to hookworms in animals which had been given repeated infections. When they were turned over to us for the diet experiments, they were all full grown, 9 to 16 months old and had very slight infestations. They were practically immune to further infection with the dog hookworm due to age and the long series of previous infections to which they had been subjected. In all five cases, the resistance was so pronounced that doses of 4,000 or more infective hookworm larvae produced no increase in their worm burden, as measured by eggs in the feces. In one case a single dose of 500,000 infective larvae resulted in no increase in egg production at a time when eggs in the feces could only be detected at all by the most careful examinations.

These five animals were all placed on the deficient diet at the same time. In spite of treatment, they all harbored a few worms at the beginning of the experiment. Two of these dogs with no additional infections began to show a considerable increase in daily output of hookworm eggs in their feces after about ten weeks on the poor diet. This was interpreted to mean that worms already present were enabled to produce more eggs as the host was affected by the poor diet. This same thing was shown by three other dogs in a later experiment not included in this paper. These dogs had been given infestations while on a good diet. After the curves of the egg counts had gone down to a low level in the natural course of the infestation, they were placed on the deficient diet and were given no further doses of larvae. Not long afterwards their egg counts increased very considerably indicating an increased egg production in the worms that remained. This finding fits in well with some of McCoy's results in which he found that the egg production of worms in resistant dogs was about one third of those in susceptible dogs.

After these first two dogs had shown the increased egg production on the deficient diet, they were treated until negative. Then after twenty weeks on this diet they were each given 500 infective hookworm larvae by mouth. After a normal prepatent period, they became positive and their egg counts rose rapidly reaching peaks of egg production comparable to those previously reached when they were susceptible puppies. In fact in one case the egg production was much greater than that produced by the earlier infections. These two dogs were transferred to the good diet after 149 days on the deficient diet with the egg output still at the peak. They rapidly regained their weight and general health and there was a rapid reduction in daily egg production, the egg count curves falling almost to zero in a period of only four weeks on the good diet. Numbers of worms were spontaneously lost after the egg production had dropped to a few thousand eggs per day. This phenomenon is suggestive of a dietary cure of hookworm in dogs. Following this spontaneous cure, repeated doses of larvae failed to produce any significant infestations showing that resistance to hookworms was regained.

The other three dogs on the first experiment were handled somewhat differently. Although previously treated, they still harbored a few worms when placed on the deficient diet. After they had been on this diet for ten weeks, they were each given a single dose of 500 infective hookworm larvae. Again after the normal prepatent period they became positive and the egg counts rose rapidly reaching peaks of egg production comparable to those produced by infections when they were susceptible puppies. These dogs were then treated until they were negative for hookworms before being returned to the good diet. This change of diet soon increased their weight and also restored their resistance since repeated doses of hookworm larvae failed to produce significant infestations.

The point to be emphasized is that all five of the dogs in the first experiment, which had been

2 O. R. McCoy, *Am. Jour. Hygiene*, 1931 (in press).

shown to be resistant to enormous doses of infective hookworm larvae while on an adequate diet, quickly lost their resistance and developed rather heavy infestations when given 500 larvae each after ten or twenty weeks on the deficient diet. Two of them when transferred to the good diet while still harboring large numbers of worms expelled them in a short time.

A second experiment with two dogs, which were estimated to be about two years old, was carried out in a somewhat different way. These two dogs were brought into the laboratory as pregnant females and were kept on the good diet about two months before the puppies were born and while they were being nursed. During this period, they were treated and later found to be negative to hookworms by repeated examination. While still on the good diet, they were each given 500 infective hookworm larvae by mouth. Both remained negative to repeated examinations for a period of six weeks, which indicated that they were very resistant to the hookworm. At this time, they were put on the poor diet and given repeated doses of 500 larvae at intervals of two weeks, the first dose being given two weeks after they had been placed on the deficient diet. After prepatent periods of 19 and 17 days, respectively, or after 33 and 31 days on the poor diet, they became positive. The egg counts increased with each subsequent infection until the number of eggs given off per day was about 700,000 for one dog and 900,000 for the other. At this time, 90 days after they had been placed on the deficient diet, they were transferred to the good diet. The doses of larvae at the two-week intervals were continued as before. But in spite of these constant doses of larvae, the egg counts in both dogs came steadily down until they reached a low level. In each case numbers of worms were lost after the egg counts had been greatly reduced. These two dogs are of especial interest since they show first the rapid breaking of the resistance in old dogs on the poor diet, and then a cure and a regaining of the resistance when they were placed on the good diet in spite of continued doses of infective larvae.

In the experiments outlined above, a definite correlation is demonstrated between deficiency in diet in dogs and susceptibility to infection with the dog hookworm, *Ancylostoma caninum*. The undernourished condition is characterized by lowered resistance to infection, increased rate of development of the worms and increased egg production per worm. When the dogs that had acquired an infestation while on the deficient diet were transferred to the good diet their recovery of resistance was indicated by a reduced egg production of the worms present, a spontaneous loss of worms and a resistance to further infection. It seems possible from this and other evidence that a similar relation may exist between the human hookworms and their hosts. We suspect that it will be found that heavy infestations are more easily built up in people on poor diets, and that not only the effects of the worms on the hosts may be reduced, but also the worms themselves may be partially or wholly eliminated by improvement in diet alone.

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FURTHER STUDIES ON THE ADRENAL CORTICAL HORMONE[1]

The work herewith reported was done upon dogs with the adrenal cortical hormone prepared according to the method of Swingle and Pfiffner.[2] The material does not contain adrenalin in excess of 1 to 1,500,000. A series of bilaterally adrenalectomized dogs maintained in good health for considerable periods with the cortical hormone were then injected with gradually decreasing doses of extract (subcutaneous, one dose each day), the dose being changed at five-day intervals. One dog was maintained for a period of five days on 1/6 cc per kg weight per day, the others on 1/4 cc per kg weight per day without symptoms of insufficiency. We regard 1/4 cc therefore as the minimal maintenance dose for a single injection per day in dogs weighing 10 to 15 kg.

Extensive experiments have been made on the blood concentration and urinary excretion of various inorganic substances and of the nitrogenous compounds, following the injection of cortical extract into normal dogs, and into adrenalectomized animals. In the normal dogs we have been entirely unable to detect characteristic changes in the blood constituents which we have followed. Carbon dioxide content and capacity (alkaline reserve), oxygen capacity, nonprotein and urea nitrogen, creatinine, sugar, calcium, potassium and magnesium, cholesterol, lactic acid, plasma chlorides, hematocrit and plasma proteins, examined in arterial blood samples are not altered in any definite or quantitative manner. The determinations have been made at hourly intervals following injection, up to five hours and at the end of twenty-four and forty-eight hours. Stress should be laid, we believe, on the fact that we have used trained animals at rest, strictly in fasting condition. There was no change in the respiratory metabolism (oxygen consumption, or R.Q.) in a normal animal so injected within five hours or at the end of twenty-four hours. The amount of extract injected has varied

1 Aided in part by a grant from the Josiah Macy, Jr., Foundation of New York.

2 W. W. Swingle and J. J. Pfiffner, *Anat. Record*, xliv, 225, 1929; *Am. Jour. Physiol*, Vol. 96, 1931.

from 1 cc to 7 cc per kg. One animal received a single intravenous injection of 100 cc of extract and showed no changes.

Bilaterally adrenalectomized dogs which are treated with adequate daily dosage of the adrenal cortical extract do not differ in their behavior, or in the blood concentration of the substances mentioned, from normal animals. Weight is maintained, appetite, pulse rate at rest, and rectal temperature is normal, and the body skin and hair are kept in fairly good condition.

When the bilaterally adrenalectomized animal, in a good state of nutrition, with well-healed wounds and without infection, is deprived of adequate injections of cortical extract, either abruptly or by gradual reduction of dosage to less than 1/6 cc per kg daily, the first significant change we have observed is a rise in blood non-protein nitrogen and urea. This is coincident with or may precede by a few hours the refusal of food, and definitely precedes the drop in respiratory metabolism which is also a constant observation. Changes in blood creatinine do not occur until the animal is very ill. The serum potassium concentration rises steadily during the period of insufficiency. The secretion of urine diminishes markedly and there is a suppression of urinary nitrogen and urea. The chloride and inorganic phosphate excretion is suppressed when the animal begins to refuse food and if the insufficiency is allowed to go to the point where urinary secretion is very low or almost suppressed there is a diminution of creatin and creatinine excretion as well as that of injected phenol sulphonephthalein. In the earlier stages of insufficiency, however, suppression of total nitrogen and of urea nitrogen occur before the excretion of creatinine and creatin lessens. No microscopic changes are found in the urine, but small amounts of albumin are quite regularly present during insufficiency. Marked and rapid loss of weight occurs where there is diarrhea or vomiting. The muscular weakness, lowering of body temperature, characteristic gait, and psychic symptoms of dogs in advanced insufficiency have been adequately described by various writers. Lowering of the systolic blood pressure does not usually occur until after the nitrogen retention has become well established. In animals which subsequently recover, following injection of adequate amounts of extract, the oxygen consumption may drop at the time of maximum insufficiency to 20–25 per cent. below the normal value at which time the R.Q. also has fallen to 0.72–0.71. Injection of extract is then followed by a diuresis which may last for 48–96 hours and is accompanied by increased excretion of urinary nitrogen and urea, and of chlorides. At the same time the animal gains rapidly in weight and his appetite returns. The respiratory metabolism usually returns slowly and the R.Q. more rapidly to their original levels. These changes precede or approximately parallel the return of the blood non-protein nitrogen and urine excretion nitrogen to their previous values. The fall in serum potassium concentration parallels that of the non-protein nitrogen. The delay in return to normal of this blood non-protein nitrogen and urea appears to be more marked following each subsequent period of adrenal insufficiency, a phenomenon which we have also noted in clinical studies on Addison's disease to be reported later.

We are now utilizing the early change in blood non-protein nitrogen and urea as a means of biological assay of the strength of different lots of cortical extract in adrenalectomized dogs. Studies of the effect of the extract and of its deprivation upon kidney function and upon anatomical renal damage are also in progress.

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